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THE BIRTH AND EARLY HISTORY OF CRYPTOZOOLOGY

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ABSTRACT: Cryptozoology aims at a systematized search for unknown species of animals about which some testimonial and circumstantial evidence is available. The necessity of its development arose quite naturally in the Western world as soon as the exploration of all other lands seemed completed, thus apparently exhausting the potential source of numerous and important discoveries. This happened around the year 1800, and the new trend was urged by the growing positivism of the scientific establishment; this was, in fact, an *a priori* incredulity about so-called fabulous animals, an attitude which is as unscientific as blind gullibility. The story of the long gestation period of the new discipline of zoology is here outlined by one of the pioneers among those who have been instrumental in creating its methodology. For many years, in popular science articles or books, the problem of still unknown animals—when these were not simply debunked—had been limited to mere descriptions, based on either individual sightings or popular traditions. Cryptozoology really started to exist when well-supported attempts were made at a zoological identification of the relevant animals; that is, their classification, as accurate as possible, into the categories of the natural system. The period here reviewed extends from the first tentative efforts of a few dauntless naturalists of the early 1800's, through some encouraging triumphs, up to the actual naming of the new discipline by the author in the late 1950's, or more exactly, to the first appearance in print of the term "cryptozoology" in 1959.

INTRODUCTION

Almost until the end of the 18th Century, zoology did not need cryptozoology. A systematic search for animal species still unknown was then quite superfluous. Since European travelers, particularly from the 15th Century on, had started to explore and conquer with insatiable greed all "lands beyond," netting, trapping or just firing at random seemed amply rewarding in this perspective.

All naturalists, aflame with curiosity, eager to discover anything new, were then lending their ears to the vaguest rumors about animals apparently still unrecorded. They were all, in a certain sense, consumed with a cryptozoological spirit, although they did not need to build up a refined method to achieve their ends.

FROM OPEN-MINDEDNESS TO DISBELIEF

Never did the zoologists of the Renaissance hesitate to admit into their catalogs or general works every animal which was spoken of in the world, even if its dried or pickled carcass—shell, skin, skull, or skeleton—was not present in the latest cabinets of curiosities or in the newborn museums of natural history. This had, of course, a disadvantage, although only a minor one: that is, to include in the manuals of zoology the descriptions and even the pictures of some lesser-known beings, often transmogrified to such an extent by tradition that the original species were no longer recognizable. In fact, it now appears that such fabulous beasts, often called “monsters” because of their extravagant and terrifying traits, were nothing but imperfectly known animals, unconsciously mythicized by our emotions, that were more or less distorted and enhanced to fit into the pattern of sundry everlasting archetypes as reflecting our fantasies, fears, cravings, prejudices, or inner conflicts (Heuvelmans 1982, 1983).

The most fantastic of these creatures—like the unicorn and the satyr, the mermaid and the sea serpent, the dragon and the basilisk, the phoenix and the roc-bird—were listed in the encyclopedias of the 16th- and 17th-Century naturalists: Edward Wotton (1492–1555), Guillaume Rondelet (1507–66), Conrad Gessner (1516–65), Pierre Belon (1517–64), Ulysses Aldrovandus (1522–1605), Edward Topsell (1572–1625), and Jan Jonston (1603–75). But a hundred years later, because these animals were obviously soaked with myth, most of them had disappeared from the works of Carl von Linné (1717–78), who tried to systematize nature, and of George Louis Leclerc, Comte de Buffon (1707–88), who looked for the causes and reasons for its diversity. Not all of them, however. *Homo troglodytes*, for instance, a wild hairy man of nocturnal habits, and *Microcosmus marinus*, a tentacular sea monster so huge it could be taken for an island, were still found in the *Systema Naturae* of Linnaeus, at least in its early editions. And Buffon still believed there were tigers in Africa, and even an abominable woodsman kidnapping black girls—the Pongo, a most unlikely creature that we now classify as *Gorilla*.

There is nothing really surprising about all that. Fifty years earlier, a very learned and respected man, Bishop Erik Ludvigsen Pontoppidan (1698–1764), had devoted an important chapter of his *Natural History of Norway* (1752–53, 1755) to the three most popular stars of the present sea monsters' Pantheon: the mermaid, the Kraken and the sea serpent. Scandinavia being a land of legends, these could have been easily dismissed elsewhere as figments of Norse myth, but no contemporary scientists thought of casting doubts upon them. Even the most fantastic creatures from literature, art, or mythology of past times appeared, of course, much more matter-of-fact as soon as equally improbably animals were discovered in faraway countries,

or their remains unearthed from ancient deposits. So, in 1784 an anonymous booklet was published in Germany, which bore a most significant title: *Beschreibung, Ausführliche und Accurate, nebst genauer Abbildung, einiger vorhin Fabelhafter Geschöpfe, welche in der Heutigen Naturgeschichte berühmter Schriftsteller gänzlich Verändert und ins Licht gestellt sind* [Detailed and Accurate Description, Together With Their Right Representation, of Some Formerly Fabulous Creatures, Which in the Present Natural History of Divers Authors of Good Reputation Appear Completely Changed and Brought to Light].

It is only when sightings of such “monsters” were reported during the 19th Century (sea serpents along the shores of North America, colossal octopuses off the west coast of Africa, and even gorgeous blond mermaids around the British Isles) that some naturalists—armchair naturalists most of the time—eventually reacted with indignation, exaggerated alacrity, and sometimes violence against what they called “ridiculous fables,” “travelers' tales,” or “chimeras generated by simple-minded or diseased imagination.” American sea serpents were branded as “Yankee humbug.”

Such a brutal change of thinking by scientists of the time calls for an explanation.

CUVIER VERSUS JEFFERSON

In 1795, President Thomas Jefferson (1743–1826), who was as well-versed in the natural sciences as in politics, examined several prehistoric bones of a giant ground sloth, including an enormous claw, which had been found in a cave in Virginia. He accordingly named the animal *Megalonyx* (great claw), but he took it for a gigantic felid “more than three times as large as the lion.” On the strength of the concrete evidence he had, he concluded: “If this animal then has once existed, it is probable on [the] general view of the movements of nature that he still exists.” He thus felt quite justified at that time in stating: “In the present interior of our continent there is surely space and range enough for elephants and lions, if in that climate they could subsist; and for mammoths and megalonyxes who may subsist there. Our entire ignorance of the immense country to the West and North-West, and of its content, does not authorize us to say what it does not contain.” He said this because, on the rocky bank of a river, there were ancient carvings of many animals, and among them “a perfect figure of a lion,” not of a puma, and also because the existence of such a monster was supported by Indian traditions “considered as fables, but which have regained credit since the discovery of these bones” (Jefferson 1799). This was a perfect example of what a cryptozoological attitude is. Jefferson was wrong about the identity of his “great claw,” but nobody would have thought of frowning or shrugging about his general statements.

Some 15 years later, however, things drastically changed when Georges Cuvier (1769–1832), the “father of paleontology,” was rash enough to pronounce that “there is little hope of discovering new species of large quadrupeds.” About fabulous animals, he even spoke with sneering irony: “I hope nobody will ever seriously look for them in nature; one could as well search for the animals of Daniel or for the beast of the Apocalypse” (Cuvier 1812).

We had entered the 19th Century, which has been dubbed “the stupid one,” not without good reason. Henceforth, dogmatism, the power of authority—in fact, sheer authoritarianism (in a word, bigotry)—was to permeate science under the pretense of rationalism and an ill-digested version of positivism. The decision of what had to be accepted or rejected by science, which was then supposed to solve all man’s problems—and thus accordingly was deified—seemed to be borrowed from some religious creed. Time had come for pontification, scientific taboos, and academic excommunications.

In the realm of zoology, this new trend showed itself in the true dictatorships exerted by some scientists (all the same remarkable men), such as Cuvier in France, Sir Richard Owen (1804–92) in England, and Rudolf Virchow (1821–1902) in Germany.

SKEPTICISM AGAINST BOTH GULLIBILITY AND INCREDULITY

A few enlightened laymen, who could be considered as the “Prophets of Cryptozoology,” had already anticipated this shocking attitude, and expressed their fears before the close of the 18th Century. The English novelist Oliver Goldsmith (1728–74), for instance, had stated in his essay *A History of the Earth and Animated Nature* (1774): “To believe all that has been said of the sea serpent, or the Kraken, would be incredulity: to reject the possibility of their existence would be presumption.” And in 1799, the English actor and dramatist Thomas Holcroft (1745–1809) had written in a letter to a friend, after having heard of sightings of the same sea monsters: “. . . who can affirm he can mark out the boundaries of possibility? Some mariners treat these tales as absolutely false and ridiculous; others seriously affirm them to be true; and I think it is a duty to collect evidence, and to remain on this question as on many others, in a certain degree of skepticism.”

As in all matters of science, skepticism is here the key word, because it sums up the true spirit of scientific research. And it is precisely because this word has so much poise that it is deliberately or unconsciously used in a false, improper, or incorrect sense by most people, and particularly by those who could be branded as “systematic debunkers.” When confronted with new facts or ideas—whether unusual, amazing, disturbing, or embarrassing—they simply shrug and say that, being skeptics, they cannot believe such fantastic tales. But they are not skeptics at all: they are just being incredulous, which is quite different. The essence of skepticism is doubt. To be a true

skeptic, one has to doubt everything, even whether one’s doubts are well-grounded; one neither accepts nor rejects anything off-hand; one does not express any opinion before conducting a careful and thorough examination of the evidence, and moreover of *all* the available evidence. As Jean de la Bruyere (1645–96), the French moralist, once put it in his *Caractères* (1688): “. . . as with all things out of the ordinary, not within the common rules, there is a course to be steered between the credulous and the unbelievers.”

Cryptozoology is based on the simplest common sense.

THE GREATEST SCIENTISTS SUPPORT THE CRYPTOZOOLOGICAL SPIRIT

Open-minded scientists never yielded, of course, to the edicts of the 19th Century dictators of science. They went on showing real interest in all undesirable “monsters” and zealously collected every scrap of information about them. The fabled sea serpent was appearing not only in sensation-seeking newspapers during the “silly season,” as it is often said, although this is completely baseless. It was regularly mentioned in scientific literature. To check it, one has just to glance through the files of some scientific journals of that period, such as the following: in Germany, Lorenz Oken’s (1779–1851) *Isis, eine Encyclopädische Zeitschrift* from 1818 to 1834, and Ludwig Friedrich von Froriep’s (1779–1847) *Notizen aus dem Gebiete der Natur- und Heimat* from 1821 to 1846, a periodical which was taken over by his son Robert von Froriep (1804–61) as *Tagsberichte über die Fortschritte des Natur- und Heilkunde* from 1850 to 1852; in the United States, Benjamin Silliman’s (1776–1859) *American Journal of Science and Arts* from 1820 to 1835; and in England, Edward Newman’s (1801–76) *Zoologist* from 1847 to 1876, and Frank Trevelyan Buckland’s (1826–80) *Land and Water* from 1870 to 1882.

One of these editors, Newman, judiciously summarized the right attitude of true skeptics in his preface to the 1847 volume of his journal: “. . . the communications and quotations about ‘the Sea Serpent’ are well worthy of attentive perusal: it is impossible to suppose all the records bearing this title to be fabricated for the purpose of deception. A natural phenomenon of some kind has been witnessed: let us seek a satisfactory solution rather than terminate enquiry by the shafts of ridicule . . . surely it is not requiring too much to solicit a suspension of judgment on the question of whether a monster may exist in the sea which does not adorn our collections.”

Among the early supporters of what was later to become cryptozoology, the greatest names of science could be mentioned. It would be a tedious task to enumerate them all, as I did in my opus on the “sea serpent” problem (1965, 1968, 1975). Let us just recall that in Great Britain, for instance, Sir Joseph Banks (1743–1820), who became the arbiter of British science as president of the Royal Society, and Sir William Jackson Hooker (1785–

1865), one of the greatest botanists of his time, both expressed their acceptance of the existence of the "sea serpent" around 1820, as Thomas Henry Huxley (1825–95), Darwin's self-styled "bulldog," would do it at the end of the same century. In the United States, the same attitude was adopted by practically all oceanographers, from Louis Jean Rodolphe Agassiz (1807–73) to the authors of the American "bible" of marine biology, *Oceanic Ichthyology* (1895): George Brown Goode (1851–96), and Tarleton Hoffman Bean (1846–1916).

The eyes of the inquisitive naturalists were not focused only on "sea serpents" during the last century. In the monumental narrative of his travels through the equatorial regions of the New World from 1799 to 1804, Baron Alexander von Humboldt (1769–1859), the "father of physical geography," expressed his doubts about the existence in South America of a large ape, around which many rumors were afloat, but he sensibly refused to consider these just as fables: "In treating them with disdain, the traces of a discovery may often be lost, in natural philosophy as well as in zoology . . . Travelers who may hereafter visit the missions of the Orinoco will do well to follow up our researchers on the *salvaje* or *great devil* of the woods; and examine whether it be some unknown species of bear, or some very rare monkey . . . which may have given rise to such singular tales" (Humboldt 1816–26, 1852–53).

THE RISE IN GREAT BRITAIN OF SURVIVING FOSSILS

In another, somewhat different vein, free-thinking scientists soon started to question the Cuvierian dogma according to which all "prehistoric" animals belong to ages gone by, have never been contemporaneous to man, and can thus certainly not be living today and possibly explain some of the present "mystery beasts."

In 1844, for instance, geologist Hugh Falconer (1808–65), after having discovered in India the fossil remains of a gigantic turtle, *Colossochelys atlas*, wondered whether the Mesozoic reptile had not survived into historical times, giving birth to the Hindu legend of the giant tortoise which supports the elephant on which the Earth rests (Falconer 1886, Falconer and Cautley 1844).

A few years later, another British naturalist, of Flemish origin, Colonel Charles Hamilton Smith (1776–1859), surmised from the traditional legends of the North American Indians that they had actually known living mastodons and giant ground sloths. Incidentally, he also supported the theory that the unicorn was a real animal, still living in remote areas of Africa, which eventually led to the discovery of the northern race of the white rhinoceros (Smith 1848).

Still later, a third British naturalist, Charles Carter Blake, author of a manual of zoology for students but nevertheless obscure, was in his turn impressed by the traditions held by the Brazilian Indians of a gigantic an-

thropoid ape, the *Cayporé*. He wrote: "No such ape exists in the present day; but, in the post-Pliocene in Brazil, remains have been preserved of an extinct ape (*Protopithecus antiquus*) four feet high, which might possibly have lived down to the human period, and formed the subject of the tradition" (Blake 1863).

In each of these cases, scientists were not necessarily concerned with the current existence of still-unknown animals, but more with the survival of animals, known from their fossil remains, into times where they were not suspected to be living any more, even possibly into the present time. However, as these animals were, in a certain way, "hidden" to the eyes of science, to try to search for them revealed a true cryptozoological turn of mind.

The ideas of Falconer, Smith, and Blake were taken up with enthusiasm by Edward Burnett Tylor (1832–1917), the "father of modern ethnology" (now called social anthropology). In his *Researches into the Early History of Mankind* (1865), he stressed the point that, up to modern times, popular traditions have preserved the memory of some "prehistoric" animals—let us now say more precisely of animals of the Lower Pleistocene—allegedly extinct before the coming of *Homo sapiens*.

This particular side of cryptozoological research was remarkably well defined in 1886 by geologist Charles Gould, the only son of John Gould (1804–81), the ornithologist and bird portrait artist. In his very important book on *Mythical Monsters*, he wrote: "I have . . . but little hesitation in gravely proposing to submit that many of the so-called mythical animals, which through long ages and in all nations have been the fertile subjects of fiction and fable, come legitimately within the scope of plain matter-of-fact natural history, and that they may be considered not as the outcome of exuberant fancy, but as creatures which really once existed, and of which, unfortunately, only imperfect and inaccurate descriptions have filtered down to us, probably much refracted, through the mists of time."

Incidentally, the Irish naturalist Valentine Ball (1843–95) had just published two articles in 1884, in which he attempted to identify some of the fabulous creatures of the early Greek authors, such as the pygmies, the Martikhora and the Griffin (Ball 1884a, 1884b).

A new wave of thought was spreading over British zoology.

EARLY CRYPTOZOOLOGY IN GERMANY AND FRANCE

During the last century, there was also in German scientific circles a great interest in the possible survival of ancient species allegedly extinct, as well as in the exciting discoveries of new species of medium-to-large-sized animals.

In 1841, Heinrich Rathke (1793–1860), a pioneer in the science of animal development, stated that there really could be no doubt about the existence of the Norwegian "sea serpent." In 1858, Karl Müller (1818–99) devoted a

long series of articles, in *Die Natur*, the scientific journal he edited in Jena, to the tentative zoological identification of the diverse "mythical animals." And in 1877, the prominent evolutionist Fritz Müller (1821–97) discussed at length, not the traditional "sea serpent," but a supposed inland "freshwater sea serpent" (if one may venture to call it thus) inhabiting the waters of the Amazon and Orinoco basins, the Minhocão. In his scholarly opinion, this aquatic monster could well be "a gigantic fish allied to *Lepidosiren* and *Ceratodus*," the intriguing lungfishes.

In fact, this suggestion had already been supported some 50 years before, in 1830, by the French explorer and botanist Augustin François César Prouvençal de Saint-Hilaire, better known as Auguste de Saint-Hilaire (1779–1853), who was one of the earliest contributors to cryptozoology (Saint-Hilaire 1830, 1847).

Even in France, some interest had indeed been shown for "fabulous animals," and this was rather unexpected in the country which boasted about its being the cradle of rationalism, and where it is often thought that expressing incredulity is a sign of reason. Baron Henri Marie Ducrotay de Blainville (1778–1850), a professor of zoology and comparative anatomy at the Paris Museum of Natural History, was among the first "sea serpent" supporters (Blainville 1818). In 1826, a distinguished historian and jurist, Eusébe Baconnière de Salverte (1771–1839), whom François Arago (1786–1853), the physicist, considered as "one of the most erudite men" of his time, devoted a well-documented study to the dragons and monstrous serpents which had been mentioned in both fable and history (Salverte 1826, 1843). And under the pseudonym of A. Frérol, the prominent malacologist Alfred Horace Bénédict Moquin-Tandon (1804–63) reserved a portion of his book on the world of the sea to the story of the then still controversial discovery of gigantic cephalopods (Frérol 1865).

THE FIRST WAVE OF POPULAR CRYPTOZOOGRAPHY

It would certainly be unfair not to mention right away all the popular science writers of the last century who helped to make the general reader "monster-conscious."

As early as 1818 in Great Britain, an anonymous author signing himself "W." had written two very well-documented articles on the Kraken and on the "sea serpent," which were published in *Blackwood's Magazine* ("W" 1818a, "W" 1818b), which were even commented on by "W.B.," that is, William Blackwood (1776–1834), the editor himself ("W.B." 1818). The gist of these was taken over by the *Retrospective Review* of London, appeared in French in the *Revue Britannique* of Paris in 1835, and was adapted, the same year, in German by H. M. Malten in his *Bibliothek der Neuesten Weltkunde* (Library of Recent World-Science) published in Switzerland, and later reprinted in Germany in 1848. A rehash of the *Revue Britannique*

version had, in the meantime, been concocted by Jules François Lecomte (1814–64) for the *Musée des Familles* of Paris (1836–37).

In 1863, a young Fellow of the British geographical and anthropological societies, William Winwood Reade (1838–75), published his narrative of a recent trip through what he called *Savage Africa*. This book contained not only first-hand information on the newly discovered gorilla, and about many tales of unicorns, tailed men, etc., but also some enlightening thoughts deserving to be used as principles of cryptozoology. For instance: "It must be laid down as a certain principle, that man can originate nothing; that lies are always truths embellished, distorted, or turned inside out. There are other facts beside those which lie on the surface, and it is the duty of the traveler and historian to sift and wash the gold grains of truth from the dirt of fable." Also: "Incredulity has now become so vulgar a folly, that one is almost tempted, out of simple hatred for a fashion, to run to the opposite extreme. However, I shall content myself with citing evidence reflecting certain unknown, fabulous, and monstrous animals of Africa, without committing myself to an opinion one way or the other, preserving only my conviction that there is always a basis of truth to the most fantastic fables, and that, by rejecting without inquiry that which appears incredible, one throws away ore in which others might have found a jewel. A traveler should believe nothing, for he will find himself so often deceived; and he should disbelieve nothing, for he will see so many wonderful things; he should doubt; he should investigate; and then he may perhaps discover."

In 1879, the English astronomer Richard Anthony Proctor (1837–88) followed these lines in writing chapters on the sea serpent and the mermaid in his *Pleasant Ways of Science*. The celebrated Victorian naturalist Philip Henry Gosse (1810–88) took a much broader view of the question of undiscovered animals in *The Romance of Natural History* (1860). He expatiated not only upon the problem of the Kraken and of the "sea serpent," calling the latter "the Great Unknown," but also upon many other cases of nondescript beings, including the African unicorn and the South American ape, which still puzzles us today.

The numerous tales which went around the world about apparently unknown man-apes or ape-men, made popular by the Darwinian revolution, such as Hanno's *Gorillas*, Livingstone's *Soko*, a Chinese tailed pygmy called the *Fese* (read *Fei-fei*), and the *Susumete* of Honduras, finally led Philip Stewart Robinson (1847–1902) to say of Professor Huxley in his own *Noah's Ark* (1882): "... if he were only to travel tomorrow into an unknown land, I am not at all sure that he would not ultimately emerge from some primeval forest hand-in-hand with the 'missing link.'" This is probably the first mention ever made of the possible survival into our time of some ancient form of hominoid.

In the meantime, in France, Armand Landrin (b. 1844) had devoted a

whole book to the various "monsters of the deep" (1867), which was later translated into English (1875). And in Germany, Emil Arnold Budde (1842–1921) resumed the discussion of both the "sea serpent" and its freshwater counterpart, the Minhocão, in his *Naturwissenschaftliche Plaudereien* (Chats About Natural Sciences) (1891).

What must be emphasized here is that most of these open-minded naturalists or popular science writers of the last century merely reported what had been said of all puzzling animals and reached conclusions about the more or less high probability of their real existence. If their achievements were to be given a technical name, one should speak of "cryptozoography" (description of hidden animals) rather than of "cryptozoology."

FOR A MORE SCIENTIFIC EVALUATION OF "MONSTERS"

Just a handful of scientists ventured to suggest during the last century how—that is, in which zoological category—the still-unknown animals should be classified. Some, for instance, first accepted blindly that "sea serpents" were just enormous snakes. However, as soon as the first fossil remains of the huge reptiles of the Mesozoic era had been unearthed, and their outer appearance reconstructed, most adventurous naturalists related the "sea serpents" at once with the sea-faring representatives of this class, namely with the *Mososaurus*, which was like an enormous snake with paddles, and with the *Plesiosaurus*, which looked more like an outsized, long-necked turtle. As a matter of fact, this was just gross guesswork, as they relied only (and hastily) upon the general outline of the animals which had been sighted.

An English naturalist, the Reverend John George Wood (1827–99), certainly distinguished himself from all others when, while visiting the United States, he had an article on the "sea serpent" published in the June, 1884, issue of the *Atlantic Monthly*, which was the keenest ever written on the subject up to that time. In the light of a careful study of comparative anatomy, he concluded that, according to its movements and behavior, as well as to its morphology, the "sea serpent" which had so often been observed off the New England coast "belongs not to the saurians, but to a cetacean animal, which, if not an actual zeuglodon, has many affinities with that creature."

Early in the 19th Century, however, two wayward naturalists had already decided, long before the Reverend Wood, to take the matter more seriously, and to study with all the strict methods of science what was then known of the most popular sea monsters—and the largest ones at that: Pierre Denys de Montfort (1764–1820) and Constantin Samuel Rafinesque (1783–1840). They were both French-born, but the latter eventually became an American citizen.

Denys de Montfort worked at the Paris Museum of Natural History, and had been commissioned to write the volumes on mollusks in the additions

to Buffon edited by Charles Nicolas Sigisbert Sonnini de Manoncourt (1751–1821). In one of these three volumes, he described at length two cephalopods of truly gigantic proportions. He named the first "le Poulpe Kraken"; as a matter of fact, it was not a true "poulpe," that is, an octopus, but a squid, which is now known as the giant squid *Architeuthis*. The other one was "le Poulpe colossal," which was later to be described as *Octopus giganteus* by Addison Emery Verrill (1839–1926), (Verrill 1897a, 1897b), although subsequently repudiated by him as just "a piece of whale blubber" (Verrill 1897c). (*Octopus giganteus* has been more recently rehabilitated as, very probably, a true gigantic octopus by Forrest G. Wood and Joseph G. Gennaro [1971], but this remarkable achievement belongs to a later period in the history of cryptozoology.)

The second naturalist who dared to describe a sea monster according to scientific rules was Rafinesque, whom historians of science later hailed as "the most remarkable man to appear in the annals of American science" (David Starr Jordan [1851–1931]), or as "among all the naturalists who have ever worked on the American continent . . . the only one who might clearly be called a Titan" (Donald Culross Peattie [1898–1964]) (Jaffe 1944). Indeed, he was the very first one, in 1817, to write a "Dissertation on Water-Snakes, Sea-Snakes and Sea-Serpents." After studying all the then available information, he concluded that, owing to their anatomical traits, there were four different types of "sea serpents," two of them being actual but outsized sea snakes, the other ones fishes, one being apparently related to synbranchid eels (Rafinesque 1817, 1819).

Denys de Montfort and Rafinesque were so criticized, ridiculed, insulted and ostracized by other scientists because of these writings, that both finally died in appalling misery, the former in a Paris gutter, the latter in a Philadelphia garret, both at the age of 56. These two ill-fated geniuses deserve to be considered the first true forerunners of cryptozoology.

THE FAMILY TREE OF CRYPTOZOOLOGY

Born in 1916, the author is currently dubbed the "father of cryptozoology," possibly because I actually coined the term "cryptozoology," but more probably because, since 1950, I have been the first zoologist ever to devote his professional life exclusively to the study of this particular field, and to the gradual development of its methodology. Even if this title were really well deserved, it should be only fair to add that cryptozoology has had several "forefathers," and, as we just saw, two outstanding "great-grandfathers," who were crowned by martyrdom in the cause of science.

To this fatal twosome one should add the German nature philosopher Lorenz Oken (1779–1851), mentioned before as the editor of *Isis*, who did not hesitate to include in his *Lehrbuch der Naturgeschichte* (Manual of Natural History) (1815) Denys de Montfort's two species of gigantic cephalo-

Pods. He listed them under the respective names of *Sepia microcosmus* and *Sepia gigas*, thus making cuttlefishes of them, which later proved to be incorrect.

To summarize, the first quarter of the 19th Century saw the heroic deeds of three "great-grandfathers" of cryptozoology.

One should, however, not overlook here the important contributions of three naturalists who flourished during the second and third quarters of the century: Auguste de Saint-Hilaire (1779–1853), for his attempted identification of the fabulous Minhocão, as well as Roulin and Steenstrup, to whom we will return later because we owe to them the first "victories" of cryptozoology. The three of them could be considered "great-uncles" of the discipline.

In the same genealogical perspective, it is rather easy to identify the two "grandfathers" of cryptozoology, who won fame during the last quarter of the 19th Century. One of them was J. G. Wood (1827–99), who was the first to recognize the mammalian nature of most "sea serpents," although this honor could well be given also to either Roulin or Steenstrup. The other was the Dutch zoologist Antoon Cornelis Oudemans (1858–1943), who, in his epoch-making monograph *The Great Sea-Serpent* (1892), actually developed the original method which underlies all cryptozoological research.

It is incontestably the methodology which defines and constitutes a new science or scientific discipline. Just as writing pleasant stories about animals does not make the writer a zoologist, reporting encounters with unidentified beasts, or traditions about them, does not make the reporter a cryptozoologist.

Probably stimulated by the penetrating views of J. G. Wood, Oudemans wanted to get more accurate and more complete information, not only on the anatomy of the "sea serpent," but also on its physiology and behavior, and on its geographical distribution, with the purpose of trying to find its proper place, with the closest approximation possible, in the system of nature. To achieve this, he simply borrowed from the physicist and astronomer Ernst Florens Chladni (1756–1827) the statistical method which the latter had used to prove the extraterrestrial origin of meteorites. This was a truly revolutionary discovery, since these stones which were said to fall from the sky had for centuries been the laughing stock of most learned men.

What Chladni had done for meteoric stones, Oudemans did for "sea serpents." He painstakingly collected from books, magazines, and newspapers as many reports of sightings as he could find (altogether 187), eliminated the obvious hoaxes and cases of mistaken identity, and then extracted from the remaining ones all the available details, which he compared systematically, one by one, to see whether they fitted or not. Finally, he drew a sort of identikit picture of the animal, with full details about its way of life. It

accordingly appeared to be a kind of long-necked and long-tailed seal, which Oudemans described scientifically as *Megophias megophias*, the generic name having been taken, with due credit, from Rafinesque.

Cryptozoology was born at last. Oudemans' great opus can be considered as the true starting point of the new discipline, much as *Recherches sur les Ossements Fossiles* of Cuvier marks the beginnings of paleontology as a science.

Oudemans' views were soon adopted by some reputable zoologists, such as the Austrian oceanographer Emil von Marenzeller (1845–1918) as early as 1894, and later on by his Franco-Rumanian colleague Emile G. Racovitza (1868–1947), and by the leading French mammalogist Edouard Louis Trouessart (1842–1927). The latter two espoused Oudemans' thesis in 1903, after numerous "sea serpent" sightings had been made by the officers and crew of several ships of the French Navy off the coast of Indochina from 1893 on. Ten years later, the director of the National Museum of Natural History of Paris, Jean Octave Edmond Perrier (1844–1921), declared that Oudemans' book had been a "veritable act of courage" (Perrier 1913).

Already in 1899, however, a Swedish college teacher, Peter Olsson (1838–1923), had gone much further. As a "monster" was said to live in the great lake Storsjön, he made local inquiries, gathered 22 trustworthy reports, and concluded from their analysis that it was an unknown pinniped similar to Oudemans' *Megophias* which inhabited this stretch of fresh water. This was actually the first scientific approach to the case of an unidentified lake animal, long before the outbreak in 1933 of the Loch Ness "monster" problem.

THREE MAJOR SUCCESSES OF THE CRYPTOZOOLOGICAL APPROACH

It is often stated nowadays that cryptozoologists are wasting their time and energy on a wild goose chase, since no unknown animal has ever been found by them. Such yarns betray a great ignorance of the history of zoology. Long before a true method of research had been defined by Oudemans, and in any case before such a method was systematically applied, some signal victories had been achieved empirically.

The first brilliant success of the cryptozoological approach was the discovery of the mountain tapir by François Désiré Roulin (1796–1874). Here is what this traveler and naturalist disclosed before the French Academy of Sciences on February 9, 1829: "Quite a long time before I actually became fully acquainted with this second species of American tapir, I had been led to suspect its existence, not, I must say, for general reasons, but on the mere strength of old Spanish chronicles. Some of these authors lent, indeed, to the tapir [a] thick hair of blackish brown color, a character which does not fit at all with the tapir of modern naturalists . . ." (Roulin 1835).

This shows very clearly that the very first task of cryptozoology is biblio-

graphical research: the patient, sometimes dull, and always time-consuming perusal of books on travel, exploration, or just hunting, of old or exotic chronicles, and of ancient or foreign works on natural history.

When Roulin proposed to name the new species *Tapirus pinchaque* it was because the latter word was the name of a fabulous animal of Indian folklore in the Popayan region of Colombia (actually *pinchaque* or *panchique*, which meant something like "ghost" or "werewolf"). Roulin was quite convinced that the belief in this much-feared monster was based upon the rare and not well known tapir of the high Andes. He even went so far as to suggest that the white-backed tapir of India had similarly inspired the legendary griffin of Greek antiquity. And he added this apt comment, which emphasizes the second important task of cryptozoology, that is, the careful analysis of myth, legends, and folk-tales of all cultures and all races in search of "monsters" which could eventually be demythicized: "Having mentioned a *fabulous animal*, I feel bound to justify myself for entertaining the Academy with a subject so irrelevant to its conventional concerns. It is, however, obvious that this kind of investigation should not be overlooked by the natural sciences. It is, indeed, impossible to trace the history of animals through ancient times without having at every moment to strip the real facts from their halo of fables, and if we do not have to discard more of these it is just because this process of elimination has been going on unsuspected for thousands of years" (Roulin 1835).

A second outstanding success of cryptozoology, and of a much more dramatic nature under the circumstances, was the scientific description in 1856 of the giant squid by the Dane Johan Japetus Steenstrup (1813–97). This scholar had first established, on historical evidence only, that the Kraken, the tentacled bugaboo of Scandinavian folklore, and a much more incredible monster than the "sea serpent" itself, was just a giant cephalopod. In his opinion, it was not an octopus, as Denys de Montfort had surmised, but an outsized calamary like the two large specimens which, according to ancient public records, had been stranded in 1639 and 1790 on the coast of Iceland (Steenstrup 1849). The Danish scientist later suggested that another fabled sea monster, the *Monachus marinus* (sea monk) of Rondelet, was likewise based upon a large squid caught in a net on the Swedish coast in 1550 (Steenstrup 1855). When he finally laid hands on the enormous beak of another specimen, which had been found dead on the shore of Jutland, on the territory of Denmark itself, and on the slightly different beak of a specimen found afloat between Bermuda and Carolina, he described the Scandinavian species as *Architeuthis monachus* and the Atlantic one as *Architeuthis dux* (Steenstrup 1857). (For the full story of this cryptozoological feat, see Heuvelmans 1958.)

A third prominent success of cryptozoology, the discovery of the okapi, was a worldwide sensation in the early 1900's. Henry Morton Stanley (1841–

1904) had casually reported, in 1890, that the Wambutti pygmies "knew a donkey and called it *atti*." This led Sir Harry Hamilton Johnston (1858–1927), then governor of Uganda, to organize a patient and obstinate search, which would lead to the discovery of the existence of such an improbable animal as an alleged forest-zebra. It was thus named *Equus johnstoni* by Philip Lutley Sclater (1829–1913) on the basis of some striped strips of skin (Sclater 1901). It was only after a careful study of a whole skin and of two skulls that Sir Edwin Lankester (1847–1929) came to the conclusion that the *atti*, rather known locally as *o-api* or *okapi*, was not a horse at all, but a sort of short-necked giraffe, more or less closely related to a Miocene fossil from Greece, *Helladotherium*. Deserving a new genus of its own, the surviving proto-giraffe became thus *Okapia johnstoni* (Lankester 1901), and caused an excitement never known before in zoological circles, and even in the daily press.

DISCOVERIES WHICH COULD HAVE BEEN MADE EARLIER WITH THE HELP OF CRYPTOZOOLOGY

It is a fact that many species of medium-to-large-sized animals could have been discovered at a much earlier date if zoologists had realized the importance of a cryptozoological approach. This is due, first of all, to the fact that most animals are generally known by the natives of the area.

The white-backed tapir of India, for example, was officially discovered by Western science in 1816, but it had been known to the Chinese and Japanese since time immemorial as the fabulous iron and copper-eating *Mé* (Roulin 1835).

The pygmy hippopotamus was described in 1849 by Samuel George Morton (1799–1851) after a set of skulls had been first revealed to him 6 years before by a simple traveler's tale which he rather disbelieved (Morton 1844). As Joseph Leidy (1823–91) soon demonstrated, it was not just a distinct species of hippopotamus, but a very different animal deserving to be put in the new genus *Choeropsis* (Leidy 1852).

The lowland gorilla, scientifically named in 1847 by the Reverend Thomas Staughton Savage (1804–80) and Professor Jeffries Wyman (1814–74), had already been fairly well described in the early 1600's as the *pongo*, a sort of "abdominable man-of-the-woods," by the English adventurer Andrew Battel (Purchas 1625).

The giant panda, found in 1869 in Szechwan by Father Jean-Pierre Armand David (1826–1900), was already mentioned as the *bei-shung* (white bear) in a Chinese manuscript dating from the Tang Dynasty, 621 A.D. (Morris and Morris 1966).

The Gerenuk or Waller's giraffe-necked gazelle, scientifically christened in 1878 as *Litocranius walleri*, could have been described much earlier since its most unusual outline appears on a rough rock carving from the east bank

of the Nile, between 4,000 and 2,900 years old, and even, very clearly this time, on an Egyptian bas-relief dating from Ramses II's reign, in the 13th Century B.C. (Schomber 1966).

Grévy's zebra was discovered in 1882 in, strangely enough, the Jardin des Plantes, the zoological garden of Paris, after it had been presented to the French president Jules Grévy by Ménélik I, Emperor of Abyssinia. It later appeared that it had already been figured by Aldrovandu in 1642 and by Hiob Ludolf (1624–1704) in 1681 (Scherren 1905).

The mountain gorilla, the largest of all apes, was scientifically described as recently as 1903, although John Hanning Speke (1827–64), the explorer, had already heard, in 1861, from the subjects of King Rumanika in Ruanda of "monsters who could not converse with men, and never showed themselves unless they saw women pass by: then, in voluptuous excitement, they squeezed them to death" (Speke 1863).

Even the coelacanth, the so-called "fossil fish" of the Devonian period found alive in 1938, had been known for ages by the natives of the Comoro Islands, who called it *kombessa*. They not only ate its salted and dried meat, but used its rough scales in repairing their bicycle tires. Moreover, a perfectly accurate silver model of a coelacanthid, at least a century old and serving as an ex-voto, was discovered in 1964 in a fisherman's church near Bilbao, on the Atlantic coast of Spain (Sylva 1966).

THE NEW WAVE OF CRYPTOZOOLOGY

From the beginning of the 20th Century on, especially because of the sensation created by the well-guided discovery of the okapi, quite a number of natural science writers, and even a few professional zoologists, started devoting articles and portions of books to some of the "mystery" animals which were still apparently undescribed.

Mention should be made here of some popular science writers of France, such as Henri Eugène Coupin (b. 1868) (1904); Victor Forbin (b. 1864), essentially a great reporter (1920); Norbert Casteret (b. 1897), the founder of French speleology, and his wife Elisabeth (1926); René Thévenin (1877–1967), a prominent author (1931); and Louis Marcellin (1949); of some renowned Englishmen from various spheres of action, such as the big-game hunter and fire-arms expert Walter Winans (1852–1920) (1920); the athletics promoter Frederick Annesley Michael Webster (b. 1886) (1920); Captain William Lionel Hichens (1874–1940), late of the Intelligence and Administrative Services of East Africa, who first wrote under the pseudonym of "Fulahn" (Fulahn 1927, Hichens 1937); and those prolific Fortean authors, Harold Tom Wilkins (1891–1960) (1950); Alfred Gordon Bennett (1901–62) (1953); and Eric Frank Russell (1905–78) (1957); and finally of naturalists, such as the French-Canadian Henry Tilmans (1905); the Catalan Spaniard Rossend Serra i Pagès (b. 1863) (1923); and the Australian min-

eralogist Charles Anderson (1876–1944), director of the Australian Museum (1934). It was in 1931 that the famed wild animal catcher Joseph Delmont (1873–1935), after having traveled all over the world for 20 years, wrote this sentence which his personal experience makes particularly significant: "Even today there are in every part of the world inaccessible areas where there certainly exist wild creatures unknown to naturalists" (Delmont 1931, 1932).

Among the wave of 20th Century authors inspired by the problem of still undiscovered animals, the following science writers must, of course, be consulted for their deserving attempts at a synthesis of the whole question, or some of its specific cases: in Britain, Frank Walter Lane (b. 1908) (1937a, 1937b, 1939, 1941); Maurice Burton (b. 1898), then of the British Museum (Natural History) (1954, 1955, 1959); and Richard Carrington (b. 1921) (1957); in Germany, Richard Hennig (1875–1934) (1950, 1957); in the United States, Alpheus Hyatt Verrill (1871–1954), who was Professor A. E. Verrill's son (1948); Ivan Terence Sanderson (1911–73) (1947, 1948a, 1948b, 1948c, 1950, 1959a, 1959b); and, above all, Willy Ley (1906–69), who had studied paleontology and the history of zoology at the Universities of Berlin and Koenigsberg (1941, 1948, 1951, 1955, 1959).

It should be stressed, incidentally, that the versatile Canadian writer Richard Stanton Lambert (b. 1894) seems to have been the first to draw attention, in a book, to the problem of the North American Sasquatch (1954).

Some books on actual fieldwork written by brilliant laymen during the same period also deserve to be listed as important contributions to cryptozoology: Commander Rupert Thomas Gould's (1890–1948) works on the "sea serpent" (1930) and on the Loch Ness Monster (1934); Kenneth Cecil Gandar Dower's (1908–44) book on the spotted lion of Kenya (1937); Ralph Izzard's journalistic reports on the *buru* of Assam (1951) and on the Yeti of Nepal (1955); Constance Whyte's careful essay on the Loch Ness Monster (1957); and C. M. Poulsen's work on sea monsters, supervised and introduced by the Danish oceanographer Anton Frederick Bruun (1901–61) (1959).

FROM PINPOINT ATTEMPTS TO A FIRST SUMMING-UP

Strangely enough, none of these numerous authors, even the best-versed in natural history, ever thought of classifying zoologically and with some accuracy some of the sundry mystery animals they were writing about. Once again, we have been dealing here with cryptozoography rather than cryptozoology. So, a special mention should be made of some pinpoint attempts at identification made during the present century by some particularly bold naturalists or zoologists.

Carl Hagenbeck (1844–1913) was not exactly a zoologist, but as "the king of zoos," the great German animal dealer was exceptionally well-acquainted with the world's megafauna, and can thus be considered an equally reliable authority. So, he certainly impressed scientists as well as laymen when he

revealed in his memoirs (1909) that he had been receiving reports from several quite distinct sources of the existence of an immense and wholly unknown animal, "half elephant, half dragon," in the great swamps of equatorial Africa, and more especially when he added: "From what I have heard of the animal, it seems to me that it can only be some kind of dinosaur, seemingly akin to the brontosaurus."

In 1913, the British ornithologist Frank Finn (1868–1938) was so struck by the recent discovery of the marsupial "mole" (*Notoryctes*), the okapi, and the giant forest hog (*Hylochoerus*) that he wondered whether a pygmy water elephant, which had just been reported from Central Africa, could not be a kind of local tapir.

In 1926, Australia's leading mammalogists, Albert Sherbourne Le Souef (1877–1951) and Harry James Burrell (1873–1945), included in their handbook *The Wild Animals of Australasia* a fairly large striped marsupial "cat" from North Queensland on the mere strength of numerous sightings. This was to be followed by Ellis Le Geyt Troughton, Curator of mammals at the Australian Museum, in the different editions of his *Furred Animals of Australia* (1941, 1954, etc.).

In 1929, the German natural science writer Wilhelm Bölsche (1861–1939) devoted a small booklet to the legend of the dragon, which in his opinion had been inspired by the survival of some Mesozoic reptiles, such as dinosaurs, until today. The strongest card in his hand was the scientific report of the Likouala-Congo Expedition of 1913–14, headed by Captain Freiherr von Stein zu Lausnitz. In this document, the German officer had spoken with a wealth of details of the *mokele-mbembe*, an amphibious animal as big as an elephant but with a long slender neck and an equally long tail, which had been described to him by the riverside villagers of the lower Ubangi and Sanga rivers.

In 1933, a school principal of Bozen (now Bolzano) in northern Italy, Jakob Nicolussi, suggested that the fear-inspiring and fabled *tatzelwurm*, a stumpy reptile or amphibian, about 3 feet long, of the Swiss, Bavarian, and Austrian Alps, could be a European species of the genus *Heloderma*, represented in America only by the gila monster of Texas, Arizona, and Sonora, Mexico (*H. suspectum*), and the Mexican beaded lizard (*H. horridum*).

In 1934, the British entomologist Malcolm Burr (1878–1954) concluded from a careful analysis of the "sea serpent" literature that these should be classified within the class Amphibia.

In 1935, Louis Seymour Bazett Leakey (1903–72), now of *Zinjanthropus* fame, tried to explain the rumors lingering in East Africa about an alleged bear of unparalleled ferocity, known to the British colonists as the Nandi Bear, by the present existence of the *Chalicotherium*. It so happened that this strangely clawed ungulate, as big as a horse, had been contemporaneous

with the okapi during the Miocene and Pliocene periods, and could thus have survived as well in the same environment.

In 1944, Mervyn David Waldegrave Jeffreys (1890–1957), professor at the University of the Witwatersrand, in Johannesburg, reviewed extensively all African tales about mysterious "flying dragons," and concluded that "the suspicion lingers that perhaps in some hidden corner of Africa a few shy pterodactyls still lurk."

In 1945, William Charles Osman Hill (1901–78), who was to become one of the world's leading primatologists, wrote a very well-informed study of the long-haired pygmies of Ceylon (now Sri Lanka), the Nittawo, exterminated at the end of the 18th Century, but which may have had a still-living representative in Sumatra, the *sedapa* or *orang pendek*. According to Osman Hill, they both could be recent relatives of the fossil "ape man" of Java, then known as *Pithecanthropus* and now known as *Homo erectus*.

In 1947, the German zoologist and philosopher of science Ingo Krumbiegel (b. 1903) devoted a study of thorough insight to an amphibious monster of Angola, locally known as the *coje ya menia* (water lion). From the footprints it left and the terrible wounds it generally inflicted on hippopotamuses, Krumbiegel deduced that the animal had to be either a huge saber-toothed cat or a gigantic monitor lizard like the Komodo dragon, or even possibly one or another saurian surviving from the Jurassic period.

Only in 1950 did somebody finally try to give a panoramic and duly documented view of the subject of still unknown animals, if only schematically, and it is the same Dr. Krumbiegel. In a small booklet entitled *Von Neuen und Unentdeckten Tierarten* (Of New and Undiscovered Animal Species), he first listed the major zoological discoveries made during the last hundred years or so, and afterwards all and sundry unidentified animals of medium to large size about which persistent rumors had circulated in the different parts of the world (Krumbiegel 1950a).

Also in 1950, Dr. Krumbiegel reminded us in another article that some of the first naturalists to study the fauna of New Zealand reported sightings, the discovery of footprints, or mere rumors of an otter-like animal living in the waters of South Island, and which the Maoris called *waitoreke* (Krumbiegel 1950b). The presence of this animal, still undiscovered today, on a far-off island which, except for bats (and for rats and dogs introduced by man), has *no* native mammals, had inclined Wilhelm Bölsche, already mentioned above, to surmise that it could be allied to the Monotremes, or to an even older group of proto-mammals (Bölsche 1896).

So, in the very midst of the present century there was already extant not only the model study of Oudemans on the "Great Unknown" of the sea, and, among a host of more or less judicious suggestions about the zoological identity of some mysterious land animals, an equally exemplary work by

Osman Hill on one of the latter, seemingly a close relative of modern man, but even a brief summary of the whole cryptozoological situation all over the world.

THE NEW DISCIPLINE AT LAST DEFINED AND NAMED

This is where the author came in, essentially as an inquirer aiming at exhaustivity, as a synthesizer of all previous research, as a theoretician in the light of epistemology, and as the refiner of the methodology of the new discipline of zoology. I think the cryptozoological spirit was probably instilled into me during my callow youth by three major works of fiction: *Twenty Thousand Leagues Under the Sea* by Jules Verne (1828–1905), because of the meeting with a giant squid and with other sea monsters (Verne 1870, 1874); *The Lost World* by Sir Arthur Conan Doyle (1859–1938), because it supposes the survival of prehistoric animals on the top of an isolated plateau in South America (Doyle 1912); and *Les Dieux rouges* (The Red Gods) by Jean d'Esme (pseudonym of the viscount Jean d'Esmenard) (b. 1893), because it is based upon the existence in Indochina of an unknown tribe of wild hairy men or ape-men (Esme 1928). It was, however, after having read in the *Saturday Evening Post* of January 3, 1948 a rather sensation-seeking article by Ivan T. Sanderson about the possible survival of dinosaurs in Africa (1948a), that I decided to write a wishfully exhaustive book on all similar cases. I happened to have accumulated for years a great amount of information on the subject, particularly while studying zoology at the Free University of Brussels. I was, of course, not aware at that time of the imminent publication of Krumbiegel's digest, which was to prove most helpful to my work.

I worked for four full years on my project, treating the various cases—several dozens of them—quite matter-of-factly, and giving for the first time all available references on them, following established practice in scientific works. After a thorough analysis of my informative material, I attempted to classify all the relevant animals as precisely as possible. Out of this vast enterprise arose, first, a few preliminary articles, published in a popular science magazine (1952a, 1952b, 1952c, 1952d), then, finally, a large opus limited to land animals, *On the Track of Unknown Animals*, which came out in French in 1955. It was subsequently translated into a dozen other languages, which explains the impact this work has had on a worldwide scale (Heuvelmans 1958a).

However, it was only while subsequently tackling the problem of the “sea serpent,” which I was to deal with in a companion volume to the former, *In the Wake of the Sea-Serpents* (Heuvelmans 1968), that I fully realized that Oudemans' method was far from perfect, and had to be considerably enriched and refined. Above all, my honored Dutch master had made a big mistake in taking for granted that *all* “sea serpents”—i.e., all elongated marine animals of large size still unknown to science—were of one and the

same species. It had gradually become apparent to me that some of them could not even belong to the same class of vertebrates: outside of one large reptile—certainly not a snake—there seemed to be not only pinnipeds and archaic whales involved, but a host of fishes, obviously unrelated to each other. To disentangle successfully this intricate problem, I finally had to resort to the most primitive computer: the punched card system.

I tried to perfect Oudemans' simple statistical approach, only efficient for the enhancement of a single isolated phenomenon, which is rarely the case in cryptozoological matters. I thus developed a generalized method of analysis which could be used—without preconceived or prejudiced ideas—in all instances of lasting rumors or ancient traditions concerning apparently unknown animals.

For three years I studied the history of the largest cephalopods, a problem which had been partly solved by the discovery of the almost incredible giant squid (*Architeuthis*), and summarized this research in a book originally published in French in 1958 (Heuvelmans 1958b). I then devoted the seven subsequent years to the study of the “sea serpent” problem (Heuvelmans 1965, 1968). It was while doing this, during the late 1950's, that I felt the need to give a name to the totally new discipline of zoology my research implied. This is how I coined the word “cryptozoology,” the science of hidden animals, which I used extensively in my professional correspondence. It first appeared in print in 1959, when one of my correspondents, Lucien Blancou (1903–83), then Chief Game Inspector of the French Overseas Territories, dedicated his book *Géographie Cynégétique du Monde* (Cynegetic Geography of the World) to “Bernard Heuvelmans, maître de la cryptozoologie” (master of cryptozoology). The term is now widely used all over the world, and has already entered several French encyclopedias and dictionaries. But this belongs now to the flourishing period of cryptozoology, which will be dealt with elsewhere in some future article.

THE EXEMPLARITY OF THE DISCOVERY OF NEPTUNE

It should be clear by now that the essence of cryptozoology lies in the collection, analysis, comparison, sifting out and synthesis of all available information on animals still absent from our zoological catalogs. It is the final result of these activities which will help us locate the relevant animals with the greatest accuracy, to recognize them, and to teach us where, when, and how to approach them, possibly lure them into a trap or a net, finally capture them if it is only on film, or ideally establish a friendly relationship with them.

To go trekking through the Himalayas with the hope of meeting a Snowman, or diving into Loch Ness, or wading in equatorial swamps in search of living dinosaurs is of course greater fun, but it is not true cryptozoological research unless it is properly conducted, and also assisted by local peoples.

No more than dredging the ocean floor, searching the dark waters of subterranean caves, or just hunting butterflies or bugs with a net. All these are mere zoological tasks: either blind attempts, exploratory tests, or processes of control. As a matter of fact, true cryptozoological research in the field consists, first of all, in collecting more complete or more recent information from the local people, and secondarily, in looking for possible concrete evidence of the existence of the animal species one is tracing.

The ambitious aim of cryptozoology is to make one able to describe an animal scientifically *before* it has been captured or collected. Such a feat can best be compared to the discovery in 1816 of the planet Neptune by theoretical astronomer Urbain Le Verrier (1811–77). It was by studying a slight perturbation in the elliptic motion of Uranus that the French astronomer deduced from it the presence of a previous unknown planet. Le Verrier told his German colleague Johann Galle (1812–1910) exactly where to look if he wanted to see the new planet, and how bright and big it would appear. Two or three days later, Galle discovered Neptune within a degree of the location Le Verrier had indicated. Camille Flammarion (1842–1925), the popular science writer, reported that, although Le Verrier had been appointed director of the Paris Observatory for his sensational achievement, he never even bothered to look at the planet he had discovered.

It is doubtful whether a cryptozoologist would ever have the admirable detachment shown by this strict theoretician if a specimen of the unknown species he had described were ever caught or found stranded.

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(Because of the historical significance of this article, the complete names of the individuals discussed have been retained [both in the text and in the references], even when they were [or are] known more commonly by shorter, abbreviated names. —Editor.)

THE SEARCH FOR COUGARS IN THE EASTERN UNITED STATES

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ABSTRACT: The history of the demise of the eastern cougar (*Felis concolor cougar*), and evidence for its possible continued existence, are summarized. The study being reported on included wide-reaching searches for sign. Only one track and one scat that are suspected to have been made by cougars were produced. Reports of hundreds of sightings, screams, and tracks were received as the result of publicity generated by the study, but in only five cases has there been accompanying substantial evidence indicating that a cougar was present. Repeated searches of these areas to confirm the presence of cougars were unsuccessful, leading to the conclusion that the animals involved were transients, or that the original evidence may have been incorrectly interpreted. The "probability of non-detection" and the implications of this project for other areas of cryptozoology are discussed.

INTRODUCTION

Few subjects are more controversial among professional wildlife managers and biologists in the eastern United States than the potential occurrence of the cougar or panther (*Felis concolor*). Many people, including several biologists, have reported seeing cougars at close range, but none have been able to produce convincing supportive evidence of these sightings. Many cougars have been reported killed within the past 80 years, but few of these reports have been confirmed because specimens were not preserved. Even so, the federal government added the eastern subspecies to the Endangered Species List in 1973. The controversy came to a head in 1977 when several groups threatened to bring suit against the U.S. Forest Service (USFS) unless it halted the harvest of timber in Nantahala National Forest, where several cougars had reportedly been seen. Subsequently USFS and the U.S. Fish and Wildlife Service (USFWS) jointly sponsored the study being reported here. I shall discuss the history of eastern cougars and cougar habitat, and the effort that has been made during the past several years to confirm their presence. This paper updates information presented at the Non-Game and Endangered Wildlife Symposium, held in Athens, Georgia, in August of 1981.

HISTORICAL REVIEW

The eastern subspecies of the cougar (*Felis concolor cougar*) disappeared from most areas soon after settlement by European immigrants. Settlers

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feared the cougar and were reluctant to share their livestock and game with it. They were easily hunted with dogs, and many were killed. Bounties were paid for killing them in several states.

Early settlers also killed deer, the mainstay of the cougar's diet. With the exception of a few inaccessible mountain ranges and coastal swamps, deer were greatly reduced in numbers or extirpated from most sections of the eastern United States by the late 1800's. Deer did not become widespread again until they were extensively stocked and protected in the 1930's, 1940's, and 1950's.

There was, however, one notable exception to the widespread extirpation of deer. In the 1890's, George Vanderbilt purchased a huge tract of land near Brevard and Asheville, North Carolina, and protected the few remaining deer there. The herd grew large enough to experience a disease dieoff in 1908, and contained an estimated 1,000 deer in 1916 (Ruff 1938). Deer persisted, but probably were not abundant, in other isolated areas, notably in what is now Great Smoky Mountains National Park.

The Weeks Act of 1914 authorized the purchase of lands to form the National Forests. That provided renewed seclusion and protection for cougars. Hunting continued on most of these lands, but was better controlled than when it was privately owned. The Pisgah Game Preserve (part of Vanderbilt's estate) was incorporated into Pisgah National Forest.

Great Smoky Mountains National Park was established in the 1930's. Although 4,252 people were moved from the park, it was historically the least accessible large tract of land in the Appalachians, and was never devoid of deer and bear (Culbertson 1977). Hunting in the park was restricted in 1930, and halted altogether in 1934. The area was so inaccessible that about 30 percent was never logged. By the early 1900's, other large tracts of land in the East also had begun the trend toward fewer people and more deer.

There is evidence that a few cougars survived the most crucial period, which probably was before 1900 in the vicinity of Mt. Pisgah, and as late as 1930 in Great Smoky Mountains National Park. Table 1 contains all reports that I could find of cougars reported killed since 1900 in the eastern United States north of Florida (the only eastern state with a recognized panther population). Most of these reports cannot be confirmed, and, therefore, should be accepted cautiously, if at all. Several reports that are known to be false (at least two were intentional hoaxes) are included to illustrate the hazards attendant to accepting reports not supported by substantial evidence. Nevertheless, as many as 20 cougars may have been killed in or near the mountains in Georgia, Alabama, Tennessee, North Carolina, and South Carolina during the last 50 to 80 years. If cougars survived the critical period, they may have even experienced an increase in number in the more inaccessible areas as deer herds became larger and more widespread.

Several people have argued that an increase in the number of cougars

killed should have been noticed if the population had increased. The removal of permanent human dwellings, the decreased need and opportunity to prey on livestock, and the passage and observance of laws protecting endangered species could account for the low kill rate. However, if all unverified kill reports of the 1970's and 1980's (Table 1) are true, the number is impressive.

Not all observers accept a kill as evidence that a wild cougar existed. Cougars breed readily in captivity, and kittens, in demand as pets, quickly outgrow the "cute, cuddly, and easy to feed" stage, and some possibly are released into the wild. Former owners will not admit releasing the animals because release is illegal in most areas, and they do not wish to be liable for any damage they cause. Determining whether a kill involved a wild or a formerly captive (feral) cougar can be resolved only by detailed skull measurements, or by performing parasite studies, such as those done in West Virginia in 1976 (see Table 1).

RECENT REPORTS

The list of kills in Table 1 is fairly good evidence that cougars survived the most critical time, but it is poor evidence for their continued existence today because each one could have been the last survivor in that locality. Eyewitness reports still seem to be the strongest evidence of the continued existence of cougars, but there is reason to question the reliability of these sightings. Several reports, however, have been made by persons with biological training and experience, who should not be mistaken in their identification. Unfortunately, only one report by a naturalist was received in time to search for sign; in that instance I found cougar-sized scratch-hills, but only tracks of dogs and bobcats.

The need to receive reports while the sign is fresh is even more important if the person who reported seeing the cougar is untrained. I solicited fresh reports in eight national magazines, in many local newspapers and state conservation magazines, and in letters to each resource agency employee in the Southern Appalachians. Fresh reports have also been actively solicited by cooperators in Pennsylvania, Massachusetts, Maryland, Virginia, North Carolina and other states. Each of these efforts was successful in shortening the lines of communication and getting news of several sightings to the investigator while the sign was fresh, but no one has yet found unmistakable evidence of a cougar.

Some investigators regard the clustering of reports in certain areas to mean these areas contain cougars. Culbertson (1977) noticed that sighting locations in Great Smoky Mountains National Park correlated well with the clustered distribution of deer. I, too, usually make note of the amount of deer sign in the area of a sighting report. This does not mean that I consider clusters of sightings around deer herds to mean there are cougars there; the witnesses may have been seeing deer, not cougars. The problem is, how does one tell

TABLE 1.—Listing of cougars reported killed in the eastern United States, north of Florida, since 1900. Included are a few reports that were checked and found to be false, as a reminder that some people purposely make false reports (and others are mistaken even when the animal is in their possession). Many others could not be checked for authenticity.

Georgia

- 1903—Bulloch County (Statesboro). Mounted specimen displayed, photo filed at University of Georgia (Golley 1962).
 1925—Harper (1927) mentions one killed in southern part of Okefenokee Swamp.
 1927—89-year-old Bill Freeze of Ellerbe, N.C., told Downing about killing one that threatened to attack him while frog hunting between Toccoa, Georgia, and Madison, South Carolina. He was of the opinion that it had escaped from a wrecked circus train. Freeze had western cougar experience.
 Mid-1960's—Malcolm Edwards has photo of young African lion claimed to have been killed by two deer hunters near Blue Ridge. Rumored to have been a pet, but became unmanageable and was destroyed.
 1975—Nowak (1976) mentions an unconfirmed kill near Stockbridge in Henry County.
 1979—Hunters claimed photo of them with a cougar in the mountains was a hoax designed to frighten hikers away from the area. Hoax claim may have been a coverup.
 1980—Rumor of 93-lb. female being killed near Rincon (north of Savannah) could not be confirmed by enforcement agents.
 1980—Downing has second-hand report that a man in northwest Georgia has killed three there in his lifetime.
 1980's—Rumor of cougar being killed near Clayton.

Alabama

- 1921—Howell (1921) reported a specimen from near Blakely (county unknown).
 1942—Article by Hardison (1976) quotes Fred Barkalow that a 109-lb. cougar was killed in TVA lakes region of northern Alabama. Mentions that this was the same year Fontana Dam was built, and that several were flushed while clearing lake bed (200 miles from kill site).
 1948—Many authors cite one killed March 16 west of Asheville in St. Clair County. Also weighed 109 lb.
 1953—One killed at Lock 14 in Tuscaloosa (Holliman 1963).
 1956—Some confusion—Jenkins (1971) says one was killed in north Alabama near Georgia line, Golley (1962) seems to quote from *Alabama Conservation* that it was killed in Dale County in southeastern Alabama.

Mississippi

- 1900—Horan's (1976) map shows a kill in north-central Mississippi.

Louisiana

- 1905—Specimen in U.S. National Museum collected near Vidalia, Concordia Parish.
 1931—Lowery (1936) reported one killed at Waverly, Madison Parish.
 1965—One killed (preserved) near Keithville, Caddo Parish, now at Museum of Zoology, Louisiana State University.
 1980–81—Bill Yenkes' 1982 master's degree thesis mentions two cougar kills.

Arkansas

- 1948—One killed near Mena, Polk County (Nowak 1976).

TABLE 1.—Continued.

- 1969—Large male killed 6 miles east of Hamberg, Ashley County. Specimen at Museum of Zoology, Louisiana State University.
 1975—A 118-lb. male killed in Logan County. Mounted specimen in Arkansas Game and Fish Commission offices.
 1979—Phone call to Downing reported finding a dead cougar. Barkley investigated and found it was a dog.

Oklahoma

- 1968—Lewis (1969) reported one killed 10 miles west of Checotah, Oklahoma, about 60 miles west of Fort Smith, Arkansas. Skeleton is at Oklahoma State University.
 1975—Nowak (1976) cites an adult found dead near Stringtown, Atoka County, in southeastern Oklahoma.

Missouri

- 1927—Schwartz and Schwartz (1959) reported one killed.

Iowa

- 1909—Bowles (1971) listed a kill this date, but doubted its authenticity.

Tennessee

- 1929—Allen (1942) mentions one killed in the Holston Mountains, Johnson County.
 1941—Culbertson (1977) claims Glen Branam (park dispatcher) and a friend treed and killed two 20-lb. kittens near Hillis Creek in the Greenbrier section of the Great Smoky Mountains National Park. Good description. Brother doubtful.
 1970's—Photo of a black panther killed near Cosby, Tennessee, published in now defunct Newport, Tennessee, newspaper. Park ranger J. R. Buchanan and Mike Pelton examined fresh specimen and said it was a large house cat. It was mounted and displayed anyway.
 1971—Mr. Buckner of Decherd, Tennessee, killed one north of Crossville while deer hunting. Mount has no Florida panther pelage characteristics. Toenails not visible on four main toes, so may be former captive. Skull is in mount, but Buckner will not allow it to be measured.
 1981—April Fool's Day article in small newspaper in Linden, Tennessee, was obvious hoax.
 1981—Rumor of one being killed in southwestern Tennessee near Bolivar in late April was a hoax.

Kentucky

- 1960—Jenkins (1971) cites Tramel (personal communication) that one was killed and photographed near Central City, Kentucky. Rumored to have escaped from a roadside zoo.

South Carolina

- 1916—Golley (1966) mentions one killed near Camden.
 1942–43—Sass (1954) tells of one being hit and killed by a truck driven by Alan G. Broun, Jr., in Georgetown County. So large that it was difficult to drag by its long tail.
 1952—Sass (1954) lists one found mangled in road by Benjamin by M. Badger, of Charleston. Also difficult to drag by long tail. Neither specimen was preserved.
 Early 1960's—Downing talked with a man who, while living near Seneca, killed cougar (judging from good description) that was catching chickens from a tree in his yard.
 1961—Beaufort paper mentions one killed.
 1979—Black panther reported killed near Central turned out to be house cat.

TABLE 1.—Continued.

North Carolina

- 1900—David Lee's files contain good report of one trapped in a pocosin in Craven County.
- 1908–09—Culbertson (1977) talked with a Rev. Conrad who said a friend of his killed an adult and two kittens near Tines Creek, Big Cataloochee area of the Great Smoky Mountains National Park.
- 1913—Hutchinson (1979) mentions one being killed in the Great Smokies near Bryson City.
- Early 1930's—Mr. and Mrs. McCall told Downing they saw one alive repeatedly; it was then killed on Waterrock Knob near Addie, in Jackson County. Good description. Could be the Bryson City specimen above.
- 1949—Lee's files contain report of a cougar killed in a pocosin in Pitt County. Low credibility.
- 1950—Lee's files contain report of cougar killed in Tyrrell County.
- 1952—Dewalt Hyde reports one killed near Robbinsville.
- 1959—Eaton (1973) says a 79-lb. male was killed on Horse Face Mountain in Cherokee County, N.C.
- 1966—Lee's files contain a secondhand report of one killed off Highway 321 between Blowing Rock and Lenoir. Lee gave report a low credibility rating.
- Late 1960's—Lee's files contain report of cougar killed in Green Swamp.
- 1970's—Secondhand report to Downing of one being killed and carcass thrown off road between Bat Cave and Black Mountain.
- 1975—Brevard man told Dick Brown and Downing that he killed one that was molesting his hogs. The only bones found near where he left carcass were dog.
- 1978—Cougar shot and sold for \$100 near Robbinsville.
- 1979—Skull from carcass that was thought to be cougar turned out to be bobcat.
- Late 1970's—Boasted cougar kill in mountains investigated by David Allen; turned out to be a female African lion salvaged from a dumpster.
- 1980—Rumor of hunter killing cougar that stole his deer near Robbinsville.
- 1981—Bill Freeze and a neighbor saw and weighed (169 lb.) a large male killed February 4 by truck of unknown persons near Ellerbe in Richmond County. Specimen not recovered. Possible hoax.
- 1981—Danny Armstrong found skeleton near Brevard. Skull large enough to be cougar, but judging by telephoned description does not have proper dental formula; probably bobcat, but skull has since become lost.
- 1982—Skeleton found in road ditch was young African lion.

Virginia

- 1978—Unsubstantiated rumor of one being poached in Shenandoah National Park.

West Virginia

- 1950's—Larson (1966) mentions three hoaxes involving Mexican and western cougars.
- 1976—One killed and one captured had parasite loads and behavior suggestive of recent captivity.

Maryland

- 1913—Shoemaker (1917) mentions one killed a few miles north of Washington, D.C.
- 1920's—David Lee has photo and data on cougar kitten shot in Garrett County. Specimen now at the U.S. National Museum.

TABLE 1.—Continued.

Pennsylvania (reports compiled by Helen McGinnis)

- 1901—Shoemaker (1943) reported that Delbert Reynolds saw one caught in a trap and shot it in Tioga County.
- 1902—Shoemaker (1943) tells of a very old cougar being shot on Scootac (Tangascootac Creek? Clinton County) while attempting to steal hams at a lumber camp.
- 1903—Shoemaker (1943) tells of another cougar killed on Scootac by Earl Monaghan.
- 1905—Shoemaker (1917) lists a "doubtful newspaper report" for Clinton County but does not mention it again in the 1943 article.
- Early 1900's—Charles Thomas, now deceased, told Bob Webber details of a cougar his relatives killed near Orviston and Monument.
- 1914—Shoemaker (1917, 1943) cites the *Lewisburg Journal* account of one being killed on Paddy Mountain (Union County?).
- 1916—Shoemaker (1917) listed two unconfirmed kills in Mifflin County, but may have determined that these were not reliable since they were not on his 1943 list.
- 1918—Niece of Willard Smoyer (deceased) told McGinnis of seeing mounted skin of animal her uncle had killed.
- 1921—Henry Masker told McGinnis he killed a young panther, and speculated that it escaped from a nearby circus.
- 1946—Carload of deer hunters, with 6-foot-long cougar tied to the fender, passed Robert Frazier between Snowshoe and Renova.
- Late 1940's—Game protector Theodore E. Carlson (now deceased) shot cougar, probably in Elk County, and told two fellow employees. One of them saw the skin.
- 1940's—Captive cougar escaped near Kane and was shot by state patrolman several miles away.
- 1967—Small (48 lb.) young adult female killed by John D. Gallant near Edinboro. Specimen (Carnegie Museum #59525) described by Doult (1969). Deformities suggest former captivity. Larger companion escaped.
- 1984—Carcass reported to Stan Gordon was a dog.

Massachusetts

- 1927—Leo Daly has photo of one killed in Shutesbury. Reportedly had a tufted tail but is obviously a cougar.

Maine

- 1906—Jackson (1922) mentions a kill near Mount Kineo.
- 1915—One killed near South LaGrange (Wright 1972).
- 1938—One killed at Little St. John's Lake (Wright 1972).
- 1949—Skull of one killed this date submitted to Safari Club International.
- 1974—A truck driver told Virginia Fifield that his uncles killed two cougars, including a 220-lb. male, near Rangeley.

Ohio

- 1966—McGinnis files mention a tame cougar shot the day after it escaped.
- 1960's—East (1979) mentions young cougar killed by car near West Virginia line.

Wisconsin

- 1903—Jackson (1961) listed one killed in Barron County.
- 1908—Jackson (1961) listed one killed in Douglas County.



FIG. 1.—A track found in a mudpuddle in West Virginia. It has many characteristics of a cougar, but is not distinct enough to identify with certainty.

which groups of sightings are most likely to contain “true” reports when the “fog” of false reports in an area that actually contains cougars may approach 90 percent? Unless several of the sightings are reported by persons of proven ability, there is no way to tell the promising areas (nine of 10 are mistaken) from the unpromising ones (10 of 10 are mistaken).

Audubon and Bachman (1851: 309), after recounting several reports of cougar attacks on people, said these “. . . must be received with some caution, making due allowance for a natural disposition in man to indulge in the marvelous.” I am not aware that any of the reports I have received were purposely fabricated or embellished, yet I am intrigued by the tendency of witnesses who get only a fleeting glimpse of an animal to make a split-second interpretation that it was the rarest animal in the East (a cougar), rather than a common one (dog or deer). Several individuals have told me that their first identification of an approaching animal was dog or deer, and that they had to change that impression as the cougar came closer. To these cases, I assign a high probability of accuracy. I also assign a high probability to those few cases (less than half a dozen) in which the witness told me about seeing the tail flicking, since this is a behavior that is seldom publicized. Most people apparently know that a cougar’s tail is carried low and turns up at the end, so it’s not surprising that a majority of reports contain this detail. Some black animals, such as Labrador retrievers, are reported to be black



FIG. 2.—Track in snow of a long-tailed cat flushed from a tree in West Virginia. If some of the heel pad was subsequently covered with snow, this would be an authentic cougar track (photo: Jim Rawson).

panthers because that’s the color that panthers are supposed to be, according to folklore (the Asian “black panther” is actually a melanistic leopard).

I regard reports of cougar screams as the least reliable evidence. House cats, bobcats, foxes, owls, and others can make the woods ring with sounds on a still night. Three people have sent me tape recordings of screams, and they all sounded exactly like grey foxes barking. People often hear a sound that is unfamiliar to them and, instead of associating it with a common animal, they report it as the rarest of all, the cougar. However, one person heard my tape of a cougar in heat, and said it sounded exactly like screams that she had heard in Maine.

In many instances, people found tracks and reported them as being those of a cougar, although the animal was not seen (Fig. 1). In more than 60

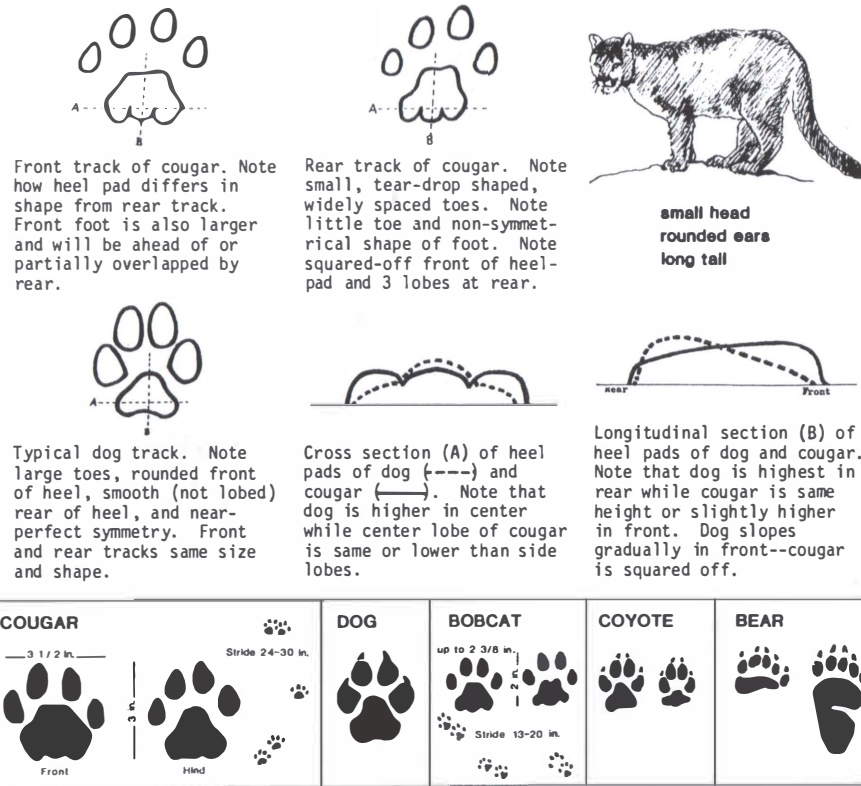


FIG. 3.—Diagrams of tracks of cougars and other animals easily confused with them were circulated widely.

instances where I saw a suspected cougar track (or a photo or cast of one), all but four turned out to be of dog, bobcat, or bear, or were too indistinct to make a determination. The four remaining ones (all photographs), from Pennsylvania, West Virginia (Fig. 2), Massachusetts, and Connecticut, look somewhat like cougar tracks. Both sites have been searched repeatedly by cooperators, and no additional cougar-like tracks have been found. I circulated about 1,000 diagrams and descriptions of cougar tracks (Fig. 3), and about 150 sets of plaster casts to familiarize key biologists and other interested persons with their characteristics. These were used to screen out many of the dog tracks before they reached me.

I cannot overemphasize the importance of *thoroughly* learning the difference between the tracks of dogs and cougars. A degree in wildlife biology is no guarantee that one knows the difference, and at least three prominent mammalogists have been mistaken in their track identification (evidence in my files). If these prestigious gentlemen could make such a mistake, so can



FIG. 4.—Covered deer kills such as this one are always the work of a large cat. Unfortunately, bobcats are also capable of killing, dragging, and covering a deer in the same manner as a cougar.

we all. In my opinion, it is far better to study plaster casts than to study drawings or photographs.

Several people sent me photographs of covered deer kills, and I myself saw six covered kills that had characteristics suggesting that a cougar was the predator responsible (Fig. 4). While nothing that I have found in the literature suggests that a bobcat is capable of dragging an adult deer, or reaching farther than 70 cm to obtain covering material, apparently some bobcats *do* have this ability, at least according to biologists to whom I offered this evidence for comment. Bobcats capable of these feats may be rare, but they probably are more numerous than the supposed cougars in the East; thus, such evidence is inconclusive, unless a good track can be found in the vicinity (Fig. 5).

Early in the study, I became aware of the potential for identifying the inhabitants of an area by chemical analysis of the bile steroids in their scats (Major et al. 1980), and I supported an effort to refine the technique. The method presently is not sufficiently accurate to be useful—29 percent of *known* bobcat scats were identified as cougar (Johnson et al. 1984). Scats larger than 30 mm in diameter and containing deer hair and bone (Fig. 6) provide sufficient reason to look for other sign, such as tracks; but scat size and composition in themselves are inconclusive evidence of cougar (Clintine 1981).



FIG. 5.—The author preparing a track plot in the vicinity of a covered deer kill, in an attempt to determine the kind of cat responsible.

SEARCHES FOR SIGN

Before the present study was begun in 1978, National Forest personnel in North Carolina enlisted the help of David Lee, of North Carolina State Museum, to compile and investigate cougar reports statewide (Lee 1977), and Dick Brown of the University of North Carolina at Charlotte to do a detailed analysis of reports from Uwharrie National Forest. Both investigators recommended further field searches for sign, and a professional cougar hunter from Colorado was brought in for a month in 1977 for this purpose. My own study was organized after this effort failed to provide positive evidence of the presence of cougars. The major strategy was to solicit and investigate fresh reports and to search for tracks in remote areas during snow. Snow was emphasized since many of the roads are paved, and dirt roads in the area seldom provide a useful tracking medium due to their hard, rocky composition.

Only one possible cougar track was found in snow (at Blue Ridge Parkway, North Carolina). The track had been covered with new snow, so the only clues were the long distance between tracks, and the fact that the animal had walked logs and wooden guardrails at every opportunity (Fig. 7). In one instance, it had jumped almost 2 meters from one log to another. The usual distance from track to track was similar to a bobcat's 40–50 centimeters, but when crossing the highway (several times) it had a distance of 65–80



FIG. 6.—Suspicious looking scat found in West Virginia which could not be positively identified.

centimeters, which is beyond the range of any bobcat that I have measured under similar conditions. However, repeated searches in the same area revealed only bobcats, and no more “long-stepping log-walkers.”

Snow does not necessarily provide a good tracking medium. Snowfall in the Southern Appalachians, where I expended most of my effort, often occurs only at high elevations; and this usually is followed by several days of high winds and bitter cold. Deer usually move to lower elevations where it does not snow as often. They seem to do most of their feeding before and during the snowstorm, not in the fresh snow afterward. By the time they become active again, the snow normally has either blown away, melted, or developed a hard crust. Bobcat tracks are abundant in fresh snow, and I am told that cougars readily walk in snow, too, so I cannot explain why I have not seen cougar tracks.

Much of the dirt tracking I have done has been in the Piedmont and Coastal Plain. This is because conditions are rarely suitable in the mountains. Road surfaces are soft enough to register tracks in the mountains only during the spring thaws. At night, when cougars usually prowl, these roads usually freeze hard again, preventing further tracks and deforming those already present. Mid-summer dust is a rare commodity in the mountains due to the frequent severe thunderstorms. An indistinct track found in a mudpuddle in West Virginia, when considered with other evidence (nearby scat, sighting



FIG. 7. —The characteristic saw-toothed pattern of tracks along snow covered logs is obvious evidence of a cat, but often the tracks are not distinct enough to tell which kind of cat was responsible.

by highly experienced biologist the year before, and recent predation on sheep), suggests the presence of a cougar there. However, a more distinct track will be needed to prove it.

Several western researchers and professional hunters have suggested that the most efficient way to determine if cougars are in an area is to look for their "scratch-hills," piles of leaf-litter that are scratched up and urinated upon by males. These scratch-hills may persist until the next leaf-fall, and therefore become more or less permanent testimony of the presence of a male cougar.

In many parts of the Appalachians, there are other species that disturb the leaf-litter and make what appear to be scratch-hills. Wild boar are perhaps the worst offenders, followed by bobcats, ruffed grouse, turkey, black bear, dogs, squirrels, skunks, and foxes. It is frustrating to search for scratch-hills in Great Smoky Mountains National Park, for example, because numerous wild boar have rooted the leaves into what looks superficially like a continuous series of scratch-hills. I have seen cougar scratch-hills in Florida and Colorado, and feel that, in paired comparisons, I can differentiate between these and hog rootings in most instances. Nevertheless, repetition tends to dull one's perceptions, and after a day of viewing several thousand hog rootings, it is possible that I could stumble over an intermingled cougar

scratch-hill without even noticing it. Except where there are boar, scratch-hill-like disturbances are uncommon enough to attract one's attention and arouse one's curiosity; but since bobcats occasionally make scratch-hills that overlap in size with those of cougars (Clinite 1981), such markings are not conclusive evidence of the presence of cougars.

Can it be said that an area that does not contain scratch-hills does not contain male cougars? According to discussions at the Mountain Lion Workshop (Nevada Fish and Game Department 1976: 89–90), there are cougar-occupied areas in the West where few, if any, scratch-hills can be found. One hunter, who had caught 31 cougars (or pumas or mountain lions as they are known in the West), had never seen a scratch-hill in the area; another who had caught 18 had seen no more than half a dozen scratch-hills. Areas with high-density, stable cougar populations generally have many scratch-hills, whereas exploited populations, composed mainly of transients and sub-adults, may not. Eastern populations, if they exist, are certainly low-density, and therefore may not be expected to contain many scratch-hills. If there truly was a cougar killed in Richmond County, North Carolina, February 4, 1981 (Table 1), North Carolina cougars do not make many scratch-hills. My wife and I spent six days during mid-February and early March walking out the likely travel-ways within five miles of the alleged kill site, and failed to find any evidence of cougars, past or present. A heavy rain had fallen between the kill date and our first visit, so tracks were not expected; but if the cougar was a resident, and had left abundant scratch-hills like the researchers elsewhere say they do, we should have found some. The function of scratch-hills is not entirely clear, but if it is to warn away neighboring males, there may be no incentive to mark if there is no neighbor.

Two behavioral traits noted in captive cougars may tend to conceal sign. Silvio Martinat, of Lenoir, North Carolina, has noticed that his captive cougars usually defecate while dipping their hindfoot in their water buckets. He interprets this behavior as a desire to defecate in water. Because there is much open water in the East, cougars would not have much difficulty hiding their scats. However, Chris Belden, of the Florida Game and Fresh Water Fish Commission, finds plenty of panther scats in Florida where water is accessible.

I once tried to get one of Silvio Martinat's cougars to walk through a mud-puddle that I had prepared. After daintily testing the wet area with its foot, the cougar, which was on a leash, could not be forced to step in the mud. If they normally act that way, we may have a problem; but Chris Belden feels that the behavior of captive cougars is seldom manifested in wild ones (personal communication). Belden has observed similar mud-aversion behavior in captives, but says wild ones do not hesitate to step in the mud. During the rainy season, wild Florida panthers actually have little choice.

Catnip has been used to attract cougars to camera sets or track plots by

Young and Goldman (1946). One reason I have not tried catnip is that I cannot find a source of strong-smelling catnip oil, and the synthetic products now on the market do not contain the proper compound, nepetalactone. Since catnip grows wild throughout the East, wild cougars here may not respond to it in any case. Several states have used urine-scented plots to monitor populations of bobcats and other predators, but none has reported cougar tracks in the plots.

CONCLUSIONS AND FUTURE PROSPECTS

Despite several years of intensive effort funded by the U.S. government, the author has been unable to positively confirm that there are self-sustaining populations of cougars in the eastern United States north of Florida. I doubt that public agencies will fund further searches unless indisputable evidence is produced. As long as there are witnesses who claim to have observed cougars, however, there will be public demands that conservation agencies take special steps to protect cougars from the public—as well as to protect the public from the cougars. At one point, the U.S. Forest Service was actually threatened with litigation if it did not halt timber harvesting on a 29,000 ha tract of land, and it would not be surprising if, in the future, petitions were made to close the hunting season in those counties in which cougar sightings are reported.

Public agencies have adopted the defensive attitude that they will not consider such radical measures until breeding populations of cougars have been proven to exist. All that I have been able to prove in six years of effort is that proof of their existence is difficult to obtain. One may wonder what resource management agencies would do if the courts were to begin accepting eyewitness reports submitted by the agencies' antagonists as proof of the cougar's present existence. As most readers of this journal are already aware, "non-occurrence" is virtually impossible to prove conclusively, so there is probably little that resource management agencies could do to counter such measures. In this connection, an article to be submitted to *Cryptozoology* (Guynn, Downing, and Askew, in preparation) addresses the theoretical aspects of calculating the "probability of non-detection." Readers are urged to test this theory, when it is published, on various cryptic animals, in order to determine its practical utility.

The future prospects of demonstrating conclusively that breeding populations of cougars exist in the eastern United States are not good. Most of the evidence is questionable at best. Some of the evidence is strong, particularly sightings by trained biologists or conservationists, but it is insufficient in itself as proof. The animals, if they exist, must be rare, elusive, difficult to track, and do not maintain breeding territories. The matter will probably remain unresolved until definite kills, substantiated by evidence that the

animals in question were not released pets, convince biologists and conservation agencies that the cougar exists and breeds in a wild state in the eastern United States.

As to which subspecies would be involved, that is a matter which can only be resolved at that time. Three possibilities actually exist: 1) the persistence of the original *Felis concolor couguar* subspecies (meaning complete extinction in the East did not occur); 2) migration of other subspecies from the West or from Florida; or 3) individuals or groups of individuals released into the wild and now breeding (such releases may have occurred at different times in different places and may have involved different subspecies, perhaps even some non-U.S. subspecies). To complicate matters further, any combination of these three possibilities may, in fact, be occurring, as they are not mutually exclusive.

Thus, the controversy cannot be laid to rest simply by officially declaring that the original subspecies is extinct. The ninety percent (or more) of the people who mistakenly report seeing cougars will continue to make the same sincere but mistaken reports, undaunted by official proclamations. Even if all biologists became convinced that there are no wild cougars in the East at the present time, there is the distinct possibility that an illegal introduction or a dispersing pair of another subspecies from Florida or the West could perpetuate the controversy at some future time. Conversely, if a few wild cougars could be confirmed today, they could all die tomorrow due to poaching or disease. If the presence or absence of cougars was as easy to determine as some investigators think, there would be no problem, and we could monitor their status from one place to another on a continuous basis.

I must conclude, unless we accept the unlikely possibility that *everyone* who reported seeing a cougar in the East was mistaken, that the animal has existed in small numbers in several areas at least within the last ten years. I do not know if it still exists at the present time, although reports continue to come in, and I cannot say if it will exist at some future time.

This situation is one with which those involved in other aspects of cryptozoology are familiar. In fact, if it is ultimately demonstrated that wild cougar populations exist in the East—and may have existed all along—this would have serious implications for other areas of cryptozoology. If it was so difficult to prove the existence in one area of an animal already very well known and studied in another area, how much more difficult must it be to prove the existence of other cryptozoological animals, which are perhaps even more elusive and of which practically nothing is known. Cryptozoologists working on other problems should thus take heart that the eastern cougar has not yet been proven to exist: it adds weight to their own cases by demonstrating, despite all arguments to the contrary, that it is very difficult to locate rare, elusive animals in wilderness areas.

ACKNOWLEDGMENTS

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"SEILEAG": THE UNKNOWN ANIMAL(S) OF LOCH SHIEL, SCOTLAND

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ABSTRACT: An animal or animals with similar morphological characteristics as those reported for Nessie may inhabit Scotland's Loch Shiel. Evidence to support the existence of "Seileag" is scarce, due, perhaps, to the remoteness of the lake. Nonetheless, recorded sightings at Loch Shiel, and sightings of similar animals in the marine waters adjacent to Loch Shiel, suggest the loch may be the restricted or temporary habitat of large, unidentified animals.

Reports of large, unidentified animals observed in Scottish inland waters are not restricted to the more publicized Loch Ness and Loch Morar (Zarzyński and Meaney 1982). Seileag—pronounced "Sheelack"—is another of these supposed large aquatic animals which are seldom observed but occasionally mentioned as residents of Loch Shiel, Scotland's fifth largest loch, located in one of its most secluded regions.

I first visited Loch Shiel in 1979 while traveling by car through the village of Glenfinnan, at the northeastern head of the loch, on my way to Loch Morar. Glenfinnan is the site where Bonnie Prince Charlie landed and raised his standard prior to his defeat at the Battle of Culloden in 1746. A monument commemorates the event (Fig. 1). I passed by Loch Shiel via Glenfinnan again in 1981 and 1982. However, it was not until 1984 that I was able to spend more time at Loch Shiel and more thoroughly investigate local sightings of Seileag.

Loch Shiel is 17 miles long (Murray 1968), and is between 100 yards and a mile wide (Atkinson 1983). Its bed is very irregular, alternating between deep and shallow. Although more than half of Loch Shiel's depth is less than 100 feet, its maximum depth, located 4 miles west of Glenfinnan, is 420 feet. Deep water is often found close in to shore, while, at the widest part of the loch, there is an expanse in the middle which is reportedly only 2-3 feet deep (Weir 1980).

Loch Shiel's weather can discourage visitors who might otherwise intend to have a lengthy stay there. Normally, the weather is wet except for May and June, when it is drier and not as windy (Weir 1980).

To thoroughly appreciate why very little is known of the reported animals in Loch Shiel, one need only survey the region's topography. Loch Shiel is a bow-shaped loch running from Glenfinnan in the northeast to the village of Acharacle in the southwest (Fig. 2). Its center line is the boundary between Ardgour and Moidart, and between Argyll and Inverness-shire (Murray 1968). Rugged, mountainous terrain hems in the loch's northern two-thirds, making



FIG. 1.—A view of Loch Shiel from the northeastern end. The Glenfinnan monument to Bonnie Prince Charlie is visible in the lower right hand corner.

it little more than a sliver in shoulder-to-shoulder mountains. Thus, it is little wonder that it lies in a region called *na garbh chriocnan* (Gaelic meaning "the Rough Bounds"). This name accurately describes the area—inaccessible, with rough, broken terrain (Gunn 1983).

The loch's sparse population also contributes to its isolation. Glenfinnan is a village of only 60-70 people. Acharacle is a scattered village, just a little larger than Glenfinnan. These are the only "major" population centers near the loch—except for Dalilea, a lochside niche with a few cottages, and several isolated seasonal cottages along the loch.

Access to the loch by road is to Glenfinnan via Route A830, and to Acharacle via Route A861. Glenfinnan is also a stop for the West Highland Railway from Fort William to Mallaig. This stretch was opened in 1901 (Gunn 1983).

The loch's shoreline was roadless until 1965-66, when the Forestry Commission constructed a 12-mile-long dirt road from Glenfinnan to Polloch on the Ardgour (south) side (Murray 1968). This road is not open to private vehicles, and a locked gate extends across it at Glenfinnan, thus preventing

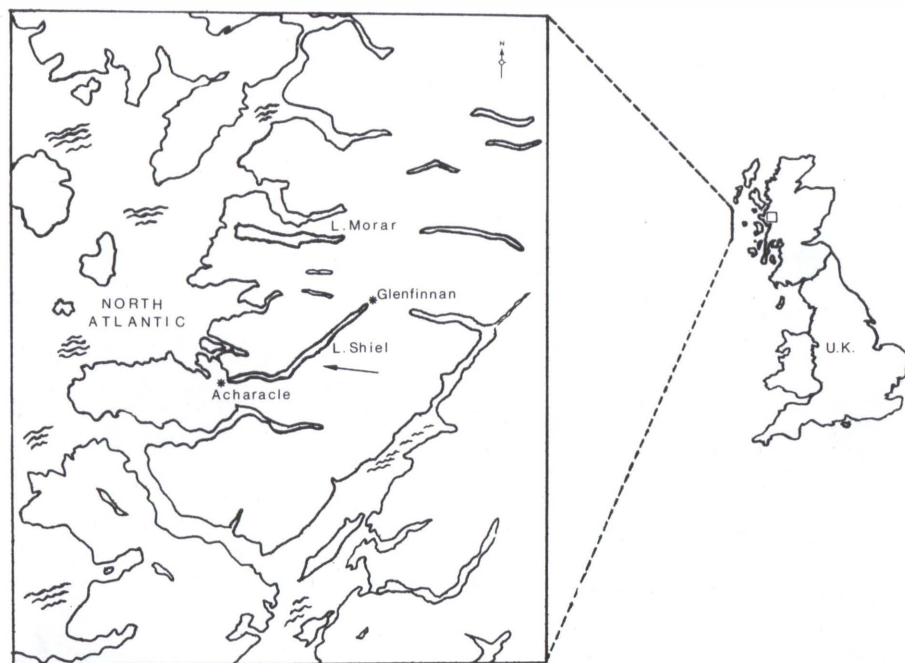


FIG. 2.—Loch Shiel (see arrow) is located in the western Scottish Highlands, a rugged and remote area.

nonofficial vehicular traffic. Daily boat sailings were once made between Acharacle to Glenfinnan, but these ended in 1967 (Murray 1968).

Loch Shiel's remoteness, its scanty population, the lack of a public road along the loch, and the cessation of the regular ferry service upon the loch, may all have contributed to the reduced chances of a surface sighting of the supposed animals inhabiting the loch.

Nevertheless, there have been several recorded sightings. Much of our knowledge of these sightings comes from Dom Cyril Dieckhoff's notes of 1933–34, a period when he toured the Shiel region as part of his pastoral responsibilities. Father Dieckhoff, of the Benedictine Abbey at Fort Augustus, had more than a passing interest in these reports, and he recorded the sightings for posterity. Father Dieckhoff died in 1970 (Campbell and Solomon 1972), but his interest in recording sightings of Seileag has recently been taken up by Tearlach MacFarlane, a resident of Glenfinnan. While living at Loch Ness as a young man, MacFarlane had sightings of Nessie, and he recently stated: "... knowing the witnesses as well as I do, I am now convinced that Seileag is with us" (MacFarlane, personal communication).

The following list attempts to compile all the known reports of Seileag from various sources:

1) Circa 1874, a woman who lived at Gaskan saw an animal with three humps; it moved very fast (Whyte 1957).

2) In 1911, an animal was observed by two men across from Gaskan. Using a telescope, they saw three humps, each separated by water. One of the witnesses was the former head keeper of the Inverailort Estate (Whyte 1957).

3) In 1905, Ewan MacIntosh, two young boys, and an old man named Ian Crookback were out on the steamer *Clan Ranald* across from Gaskan when they sighted three humps. They observed them through a telescope (Whyte 1957, Costello 1974).

4) In 1925, the animal was seen at Rhu Ghainmheach. Three humps were observed through a telescope, the middle hump being the largest; it was described as being "longer than the little mail steamer *Clan Ranald*" (Whyte 1957).

5) In 1926, Ronald MacLeod saw an animal coming out of the water at Sandy Point, between 3 and 4 p.m. MacLeod studied it through his telescope; he told his sister, Ann Mor MacDonald, that it was bigger than the *Clan Ranald*, with a long, thick neck, a broad head with a wide mouth, and seven "sails" on its back (Whyte 1957, Costello 1974).

6) In December 1933, Father Dieckhoff was informed by an old man who lived by the Shiel bridges of a sighting near Dalilea—date not recorded (Whyte 1957).

7) Sandie MacKellaig tried to shoot an animal (described as being a Seileag) from his boat while giving passage to two tinker women. The women admonished him saying, "Do not interfere with it, it has not done you any harm"—no date recorded (Whyte 1957, Costello 1974).

Tearlach MacFarlane has catalogued two more recent sightings at Loch Shiel.

8) In the 1950's, an animal was sighted about 6 miles from Glenfinnan (MacFarlane, personal communication).

9) In July, 1979, there was a sighting by two local youths from the pier at Glenfinnan (MacFarlane, personal communication).

In addition to the above, a review of the "sea serpent" literature reveals several sightings of animals similar in appearance to Seileag (or Nessie) being observed in Scotland's coastal waters, just a few miles from Loch Shiel (unlike some Scottish lochs, which are actually marine fjords, Loch Shiel, like Ness and Morar, is a freshwater lake):

1) In summer, 1932, a dark, grey animal was observed by a number of people from Smirissary Point, at a range of 60 yards. Its head and neck were seen. It disappeared, and then a large hump was viewed (Whyte 1957).

2) Another large, unidentified creature was observed near Arisaig in Loch nan Uamh, a fjord (Whyte 1957).

3) Circa 1900, a "sea serpent" was seen off Rhum (Heuvelmans 1968).

4) In 1965, an unknown animal was seen off Ardnamurchan (McEwan 1978).

Possibly linked with these coastal "sea serpent" sightings is the derivation of the name of Shona, for a nearby island, and a superstition associated with the island. The island takes its name from the Celtic sea god *Shoney*. Sacrifices to *Shoney*, usually of fish, were made by local fishermen up until the 1800's (Atkinson 1983). One may wonder if this ritual was somehow related to sightings of large, unidentified animals that frequented the nearby coastal waters.

This brings up an old question which is often asked about Nessie: Is Seileag restricted to Loch Shiel alone, or is it mobile, utilizing the short (3-mile) stretch of the River Shiel between the loch and the sea?

Loch Shiel and its adjacent marine waters could well become a crypto-zoological field laboratory, much as Loch Morar has occasionally been studied by groups and individuals working at Loch Ness. If so, we may one day learn if Tearlach MacFarlane's statement on Seileag is correct: possibly there is "a survivor or two in this loch yet."

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THE ORANG-UTAN IN ENGLAND: AN EXPLANATION FOR THE USE OF *YAHOO* AS A NAME FOR THE AUSTRALIAN HAIRY MAN

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ABSTRACT: The Australian hairy man or Yahoo was the subject of speculation throughout most of the 19th Century and beyond. The name Yahoo was often held to be an aboriginal word, although it was also attributed to Swift. It is suggested that the word Yahoo was used to describe the adult orang-utan when that animal first arrived in England. This appears to be the reason for its use in Australia.

Among unresolved problems surrounding the Australian hairy man to which a solution might be attempted, one of the most intriguing concerns the name Yahoo, by which the legendary creature was known to Australians of European origin throughout much of the 19th Century. The curious fact is that, on the unanimous testimony of the earliest writers who mention the matter, the name Yahoo was also used by the Australian aborigines themselves, and belonged, or so it is implied, to one of their languages. Consequently, it came to be considered an aboriginal name. How did this happen? At least one writer who was aware of the difficulty involved realized that the name was used in Swift's political satire *Gulliver's Travels*, but thought this was simply a coincidence. This is obviously an unsatisfactory conclusion, and another explanation must be sought.

In so doing, there is no need to refer to the large mass of later material describing the Yahoo reports (Joyner 1977, 1980). Instead, a brief discussion of the earliest Australian records, together with an examination of certain events in England at the very beginning of the period under consideration, will throw some light on the probable origin of the word as it was used in Australia.

The earliest known description of the Yahoo is that of 1842 in the *Australian and New Zealand Monthly Magazine*. Here the Yahoo appears as the subject of an aboriginal superstition, a hideous monster of unearthly character and ape-like appearance, although we are told, curiously enough, that there had long been contention among Australian naturalists as to whether or not such an animal actually existed. In her *Notes and Sketches of New South Wales*, published in 1844, Louisa Ann Meredith refers briefly to an aboriginal belief in the "Yahoo" or "Devil-devil" as a kind of bad spirit.

The name also appears in the 1847 book *Settlers and Convicts* by a writer passing under the name of Alexander Harris, although the reference to the Yahoo is in the summary at the head of a chapter, and not in the text itself. The latter carries only a description of a man-like animal spoken about by

the aborigines of the region of the Hunter River. This discrepancy indicates that the name was thought sufficiently well known when the book was published in 1947, but that it was not known to the aborigines around 1830, or whenever the author had actually been engaged in cutting cedar. Finally, a newspaper report from 1847 contains a reference to a carnivorous "Yahoo" known to the natives at Hunter River.

Descriptions of the Australian Yahoo, wild man, or hairy man of the woods are, it may be supposed, heavily dependent upon earlier English perceptions of the orang-utan. It is well known that this once mysterious creature was commonly called "wild man of the woods" in its own Southeast Asia habitat, but it has not previously been demonstrated that the orang-utan was probably also known by the name Yahoo. Evidence for this is provided by the following two examples. According to the *Lincoln, Rutland and Stamford Mercury* of 22 April, 1814, an exhibition in Lincoln of T. Shore's superb collection of living rarities included "the Great Yahoo, or Wild Man of the Woods," while a handbill belonging to the period 1810 to 1820, printed in Boston and now in the Ferguson Collection of the Australian National Library (Ferguson 1941), bears the intelligence that "Two surprising large Yohoes; or, Wild Men of the Woods," might be seen among the exhibits in a traveling menagerie.

An adult orang-utan had been captured in Borneo about 1780. The creature was killed because of its supposed fierce resistance, and was sent preserved in spirits of rum to the Batavian Society of Arts and Sciences. From there it may have been shipped to Holland. In any event, the Secretary of the Society, Baron von Wurmb, prepared a detailed description of this "large Orang Outang," which appeared in English in *The Philosophical Magazine* for August, 1798. Meanwhile, Geoffrey Saint-Hilaire had found a skeleton in the Museum of Natural History in Paris, which he supposed to have been that of the animal shipped to Holland by Wurmb. A translation of his description of the "large orang outang" was printed in *The Philosophical Magazine* of September, 1798. Even though Saint-Hilaire concluded (wrongly) that this was not the orang-utan but an entirely new species, the skeleton was nevertheless the first tangible evidence of the existence of a man-sized ape (Greene 1959).

The date when an adult orang-utan was first exhibited in England has never been determined. This must have occurred in the early years of the 19th Century, however, and it may be conjectured that expressions like "the Great Yahoo" or "large Yohoes" were used at the time to distinguish the size and novel appearance of the animal.

The popular use of Yahoo at this time in England was not inconsistent with its appearance in Australian literature several decades later. The otherwise unaccountable attribution of the word to the Australian aborigines

may merely indicate the facility with which the latter had assimilated a new language.

A further inference may perhaps be drawn from the knowledge that words used to describe the Australian Yahoo or hairy man are those associated with descriptions of the orang-utan, the various "wild men" of the 18th Century, and, at a later date, the gorilla. As often in Australia as no doubt elsewhere, a familiar name was applied to an alien form. There is consequently no need to assume that the hairy man was man-like, except insofar as it was large, tailless, covered with hair or fur, and capable of walking upright.

Descriptions likening it to the wombat or native "bear" may be nearer to the truth, as may be Henry Lawson's remark that the hairy man was thought by some to be the last of a species of Australian animals which hadn't been discovered yet. Perhaps the Yahoo was indeed some entirely unknown animal, like the projected Pleistocene species *Phascolarctos stirtoni* (Bartholomai 1968). Whatever the case may be, the problems are as much conceptual as technical.

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MORPHOLOGICAL ANALYSIS OF THE JIULONG MOUNTAIN "MANBEAR" (WILDMAN) HAND AND FOOT SPECIMENS

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ABSTRACT: Hand and foot specimens from a "Manbear" (Wildman) killed by villagers at Jiulong Mountain in 1957 were studied. After morphological analyses and comparative measurements with Chinese macaque monkeys, it has been determined that the "Manbear" specimen most closely resembles a large stump-tailed monkey recently caught at Huang Mountain, which has not yet been scientifically described. It is concluded that the Jiulong Mountain "Manbear" represents, in fact, an unknown species of large monkey, probably a macaque. This does not solve the problem of the Chinese Wildman, which is reported to be over two meters tall and fully bipedal.

INTRODUCTION

On the afternoon of May 23, 1957, a girl cowherd named Wang Congmei encountered a strange animal near Zhuanxian village, Shuichang County, Zhejiang Province, in the People's Republic of China. The area is near Jiulong Mountain (Jiulongshan). It was killed by her mother and other villagers. The hands and feet of this animal were severed and kept by a middle-school biology teacher. In 1981, the Scientific and Technological Society of Shuichang District reported on this incident. After learning the news, the author made an on-site, preliminary investigation. This was supposedly the first material evidence of Wildman found in China (Zhou 1982).

The witnesses involved included Wang Congmei, her mother Xu Fudi, the Bihu middle-school teacher Zhou Shousong, and others.

The witnesses all agreed that the "Manbear" did not resemble either a bear or a monkey. Its features were described as strange and like those of a man. Zhou Shousong said that the face of the animal looked like that of a human. Its head was said to be round, about 16-17 cm in length, and its mouth did not protrude, being flat. The hands and feet were shaped somewhat like those of a bear, but had nails instead of claws. The toenails were

flat. Wang Congmei said that the head was similar to that of a man, and the hairless skin was thin. They later found bamboo shoots and grass in its stomach. According to Miss Wang's recollection, the animal walked quadrupedally, with a rolling gait. The height of its shoulders was about 40 to 50 cm. The body was "strong." The walking posture looked like that of a panda. There was uncertainty concerning whether it had a tail. Wang said it had no tail, but another witness said he saw a short one. Male genital organs were visible, and it also appeared to have small breasts. There was no hair on the seat part of the buttocks, and the area included a black spot. I showed the witnesses a photo of a young orangutan; both Wang and her mother said that it resembled the animal.

According to this preliminary investigation, the "Manbear" they had killed had the following features and characteristics:

- 1) The body was over 1 meter tall when erect and about 50 cm tall when in a quadrupedal position.
- 2) Locomotion was mainly quadrupedal.
- 3) The hair was black, with curls on the head.
- 4) The head was round, the mouth not protruding, and the animal had features similar to a young orangutan.
- 5) It moved slowly, and did not initially show aggression.
- 6) It was a vegetarian, eating bamboo shoots in the spring.
- 7) There was no hair on its buttocks.
- 8) It had a short tail, or none at all.

METHOD

The hand and foot specimens of the Jiulong Mountain "Manbear" were carefully examined and measured for their morphological and biomechanical significance. X-ray images were also obtained in order to study internal osteological features. Hand and foot measurements of Chinese monkeys most closely resembling the morphology and behavior of the Jiulong Mountain "Manbear" were also obtained at zoos and museums, and these were compared with the "Manbear" measurements.

RESULTS

The physical remains of the Jiulong Mountain "Manbear" are represented by a pair of hands and a pair of feet (Figs. 1, 2 and 3). Small parts of the tibia and fibula are preserved in the right foot. All toenails of both feet are intact. Some parts of thick callus on the soles are missing. In order to observe the soleprint and the toeprints clearly, I cleared all thick callus from the left foot.

After being preserved for so long, the muscles, especially in the fingers and toes, show obvious dehydration and amyotrophy. The hair is somewhat



FIG. 1.—A bottom (palm) view of the Jiulong Mountain “Manbear” hands.

black-brown in color. The soles are red-brown, and the palm surfaces are somewhat yellow-brown.

There is no hair on the palms of the hands and feet. The skinprints (dermatoglyphic patterns) are clear, intact, and easy to compare and study.

There is thick hair covering the backs of the hands, the wrists, and the radial sides of the forearms. The longest hair is about 3.5 cm in length.

The fingers: There is hair on the backs of the first phalanges, but not on the second and third ones. There is no thickening of the skin or widening of the phalange bones, and we can thus conclude that the animal did not knuckle-walk. No obvious thick callus is visible on the palms. The distal ends of the thumbs surpass the level of the metacarpal-phalange joints, but not as far as with humans. The thumbs of the apes, on the other hand, are small, and do not surpass the level of the metacarpal-phalange joints.

The fingers are conical in shape. The finger length formula is $3 > 4 > 2 > 5 > 1$. The angle between the thumb and the axis of the palm is 19° , the same as with humans. The degree of flatness of the fingernails is great. Their shapes are similar to those of both humans and apes, but different from those of monkeys.

The backs and sides of the feet and toes are covered with thick hair. The length of hair is about 1.5 cm; the longest one is 3.2 cm. The hair is much longer at the ankle, about 6.5 cm. The thickness of the callus on the sole is about 1–2 mm.



FIG. 2.—A top (dorsal) view of the Jiulong Mountain “Manbear” hands.

The muscles of the foot show obvious dehydration and amyotrophy. Viewed from the sole, the shape of the foot is an elongated rhomboid. The heels are small, and quite different from those of humans and apes.

The big toes are large, and similar to those of man. However, they branch off sideways and are not parallel to the other four toes. The maximum abduction (spread) angle between the big and second toes is about 90° to 100° , which is a smaller angle than with other stump-tailed monkeys.

There are well-developed membranes between the toes, which reach to between the first and second phalangeal joints. Those of fingers reach only to the middle of the first phalanges.

There are several well-developed volar pads (or muscle spheroids) on the soles of the feet. These are located at the metatarsal-phalange joints of digits II through V, on the muscle pad of digit I, and on the lateral edge of the sole. Some volar pads are also visible on the hands, but they are not as well developed as those of the feet.

There is no foot arch. The toe bones lie flat against the sole of the foot. Except for the conical shape of the nail of the second toe, which is “paw-shaped” at the tip, the toenails are flat. The second toe is the smallest.

Judging from the structures of the hands and feet, it is apparent that they had different biomechanical functions. The feet were the principal means of locomotion, but could also grasp. The hands were mainly for grasping, but had the ability to support the body.



FIG. 3.—A sole (plantar) view of the Jiulong Mountain "Manbear" feet.

The following characteristics of the hands and feet were revealed by X-ray examination: all epiphysis have fused, and their lines are obliterated, so the specimens belonged to an adult. However, because there are no obvious soft-tissue scars on the specimens, I consider the animal to have been a young adult.

The bones of the wrist are small, and have large gaps between them. Furthermore, much larger gaps are visible between the distal joint surfaces of the radius and ulna, and the navicular, lunate, and triquetral bones. This demonstrates that the wrist could adequately exercise the function of spreading the palm and giving ground support in locomotion.

There is only a small joint surface at the base of the thumb—that connecting the first metacarpal with the trapezium. It appears that the movement of the thumb, relative to the palm, is limited. There is no apparent increase in the size and thickness of the second phalangeal bones. The possibility of knuckle-walking, as with apes, is therefore excluded.

The radiographs show the proportion of the phalanges, metatarsals and tarsals is 36.6/31.1/32.2. It is quite different from that of humans, which is 15/30/52, where walking erect has greatly strengthened the metatarsals. There are many sesamoids in the "Manbear" feet, indicating that they possessed

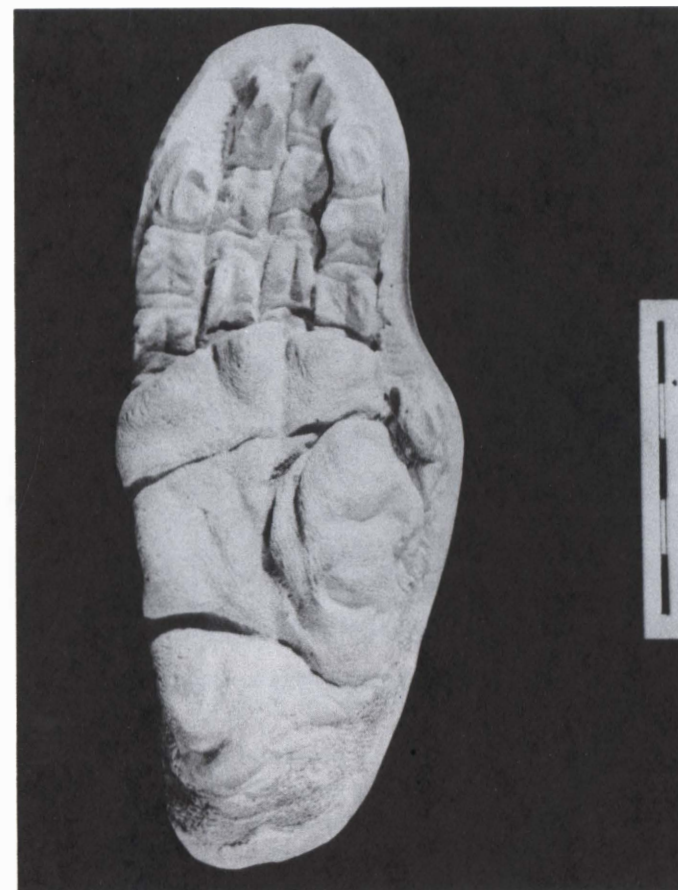


FIG. 4.—A cast of the right hand of the Jiulong Mountain "Manbear." Note the dermatoglyphic patterns.

the main locomotory function. The skin of the heel shows some wear, indicating a primary adaptation to ground-dwelling.

Let us now discuss the skinprints of the fingers and palms. The dermatoglyphic patterns of the fingers show irregular long whorls. There are three flexor creases in the palm (Figs. 1 and 4) which divide it into four parts: the distal, the proximal (near the wrist), the medial, and the lateral. There is a regular dermatoglyphic pattern spread over the entire palm. An extension of the central crease separates the distal part into two sub-parts, whereas the other parts are not divided. Extensive spiral ridges that center on the convex areas are visible.

There are three lines of flexor creases in the human palm, but it is divisible

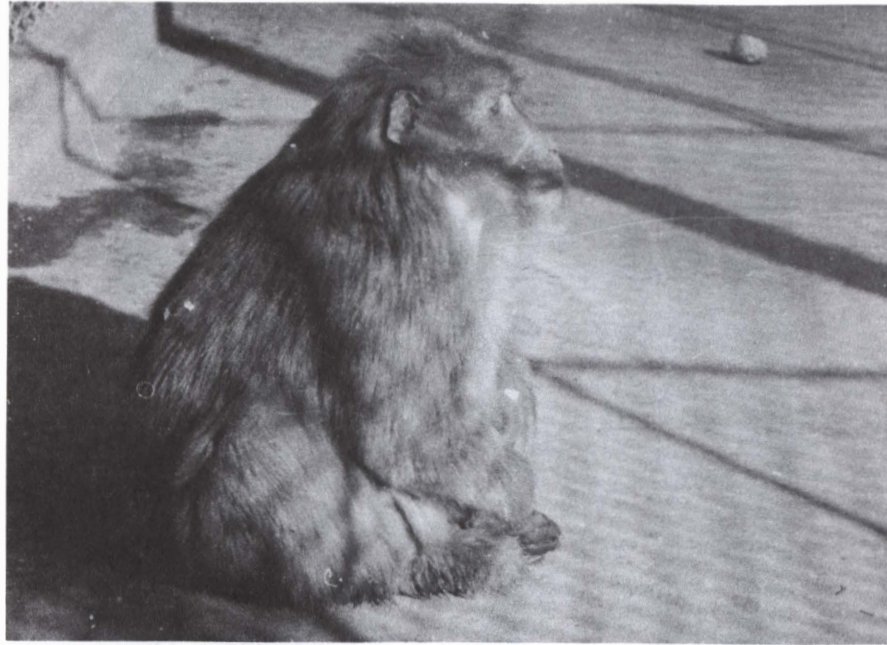


FIG. 5. — The new Huang Mountain monkey, probably a new species or subspecies of *Macaca*.

only into three parts, and no obvious proximal part is visible. The dermatoglyphic pattern of the human palm is generally arch-shaped, and the spiral-shape is rare. The prints of the fingers and palms of the "Manbear," therefore, are different from those of man as well as ape.

The characteristics of the specimens of the Jiulong Mountain animal can be summarized as follows:

1) The hands and feet show many morphological differences. The hands, besides helping to walk, were mainly used for grasping. The feet were used mainly for locomotion. The sole fully touched the ground, but the big toe still maintained the ability to grasp. This animal was mainly ground-dwelling.

2) The palms show some similarities to those of man, but are different from those of apes. They are more like those of monkeys.

3) The feet are obviously different from those of humans, but have some similarities to those of the chimpanzee. They are much closer to those of ground-dwelling monkeys.

4) Except for the second fingers and second toes, the nails are flat, similar to those of man. They show many differences from those of known macaque monkeys such as *Macaca mulatta*, *M. assamensis*, and *M. speciosa*.

5) The dermatoglyphic patterns of the palm and sole are obviously dif-

TABLE 1.—Comparisons of hand measurements (mm) of the Jiulong Mountain "Manbear," the still-undescribed Huang Mountain monkey, and two other Chinese macaques.

	Jiulong Mountain "Manbear"			Huang Mountain Monkey			<i>M. assamensis</i> (SZ) R.		<i>M. speciosa melli</i> (SZ) R.		<i>M. assamensis</i> (SNHM) L.	
	L	R	L	R	L	R	(SZ) R.	(SZ) R.	(SZ) R.	(SNHM) R.	(SNHM) L.	(SNHM) L.
1. Total length	125.0	127.0	114.0	114.0	114.0	114.0	112.0	114.0	114.0	43.5	41.0	41.0
2. Total breadth	46.0	46.0	47.0	47.0	47.0	47.0	44.0	49.0	49.0	43.5	41.0	41.0
3. Breadth-length index (2/1)	36.8	36.6	39.5	41.7	39.5	41.7	38.3	43.0	43.0	43.5	41.0	41.0
4. Length of middle finger	68.0	69.0	66.0	66.0	66.0	66.0	77.0	71.0	71.0	43.5	41.0	41.0
5. Mid-digital index (4/1)	54.4	54.3	55.0	57.9	55.0	57.9	68.8	62.8	62.8	43.5	41.0	41.0
6. Palm length	132.5	137.0	134.0	133.0	134.0	133.0	127.0	128.0	128.0	87.0	118.0	118.0
7. Thumb length	22.0	22.0	23.0	24.0	23.0	24.0	23.0	25.5	25.5	43.5	41.0	41.0
8. 1-2 Finger angle	90.0	90.0	95.0	95.0	95.0	95.0	110.0	90.0	90.0	43.5	41.0	41.0
9. Fingernail shape index of 1st*			97.2	87.5	97.2	87.5	162.0	85.7	85.7	100.0	125.0	125.0
10. Fingernail shape index of 2nd			97.2	68.0	97.2	68.0	64.1	73.2	73.2	51.0	70.0	70.0
11. Fingernail shape index of 3rd		73.3	76.1	65.0	76.1	65.0	75.0	73.2	73.2	45.5	100.0	100.0
12. Fingernail shape index of 4th	75.0	75.5	65.0	66.7	65.0	66.7	100.0	57.8	57.8	43.6	74.1	74.1
13. Fingernail shape index of 5th	62.5	61.1	52.6	50.0	52.6	50.0	69.0	62.5	62.5	47.1	51.5	51.5

* Fingernail shape index = (fingernail breadth/fingernail length) × 100.
SZ = Shanghai Zoo; SNHM = Shanghai Natural History Museum.

TABLE 2.—Comparisons of foot measurements (mm) of the Jiulong Mountain "Manbear," the still-undescribed Huang Mountain monkey, and two other Chinese macaques.

	Jiulong Mountain "Manbear"			Huang Mountain monkey			<i>M. assamensis</i> (SZ)			<i>M. speciosa melli</i> (SNHM)			<i>M. assamensis</i> (SNHM)		
	L.	R.	L.	R.	L.	R.	L.	R.	L.	(SZ)	L.	R.	(SNHM)	L.	R.
1. Total length	185.0	184.0	183.0	183.0	175.0	175.0	175.0	175.0	175.0	175.0	155.0	155.0	140.0	140.0	140.0
2. Total breadth	78.0	79.0	74.0	73.0	65.0	65.0	65.0	65.0	65.0	65.0	52.0	52.0	52.0	52.0	52.0
3. Breadth-length index (2/1)	42.3	42.9	40.4	39.7	37.1	37.1	37.1	37.1	37.1	37.1	33.5	33.5	33.5	33.5	33.5
4. Length of middle toe	75.0	75.0	56.0	55.0	37.1	37.1	37.1	37.1	37.1	37.1	38.0	38.0	36.0	36.0	36.0
5. Mid-digital index (4/1)	40.5	40.8	30.0	30.0	30.9	30.9	30.9	30.9	30.9	30.9	24.5	24.5	25.7	25.7	25.7
6. Tarsus breadth	27.0	30.0	24.5	29.0	19.0	19.0	19.0	19.0	19.0	19.0	30.0	30.0	36.0	36.0	36.0
7. 1-2 Toe angle	100	90	115	120	150	150	150	150	150	150	120	120	100.0	100.0	100.0
8. Toenail shape index of 1st*	125.0	137.5	141.1	105.6	133.3	133.3	133.3	133.3	133.3	133.3	89.5	89.5	100.0	100.0	100.0
9. Toenail shape index of 2nd	60.0	45.5	43.5	41.5	44.4	44.4	44.4	44.4	44.4	44.4	40.0	40.0	32.0	32.0	32.0
10. Toenail shape index of 3rd	82.4	83.3	60.0	58.0	68.8	68.8	68.8	68.8	68.8	68.8	55.9	55.9	43.6	43.6	43.6
11. Toenail shape index of 4th	77.8	70.1	60.0	57.1	46.9	46.9	46.9	46.9	46.9	46.9	41.8	41.8	53.5	53.5	53.5
12. Toenail shape index of 5th	58.8	62.5	46.7	53.1	47.5	47.5	47.5	47.5	47.5	47.5	38.0	38.0	40.0	40.0	40.0

* Toenail shape index = (toenail breadth/toenail length) × 100.
SZ = Shanghai Zoo; SNHM = Shanghai National History Museum.



FIG. 6.—The hand of the new Huang Mountain monkey, showing the flat fingernails. These resemble those on the Jiulong Mountain "Manbear" hands.

ferent from those of man and ape. The palm is divided into four parts by three flexor creases. The dermatoglyphic patterns are spiral.

6) Judging from the data available, the animal was large, and represented a considerable biomass when it lived.

In order to further identify the specimens of the Jiulong Mountain "Manbear," I visited the zoos and museums of Beijing, Shanghai, Hangzhou, and Hefei to make comparisons with known apes and monkeys, especially macaques. Forensic medicine specialists in Shanghai also helped me with the identification of hair microstructure, blood types, and skinprints.

Although some of the features of the specimens resemble those of an ape, the major characteristics and structures are more similar to those of a macaque monkey.

The dermatoglyphic pattern is similar to that of *Macaca mulatta*, *M. assamensis*, and *M. speciosa*, the latter two of which are found in China. *M. speciosa* could also be represented by two subspecies: *M. s. melli* and *M. s. tibetanus*. A large and new kind of stump-tailed monkey has also been captured recently near Huang Mountain (Huangshan), but has not yet been scientifically described. It probably represents a new species or subspecies



FIG. 7.—The dermatoglyphic pattern of the left hand of the Huang Mountain monkey.

of Chinese macaque, and is currently in the Hefei Zoo, Anhui Province (Fig. 5).

Comparisons of the hand and foot measurements of the Jiulong Mountain "Manbear" and the above-mentioned macaques have been made. The morphological features of the Jiulong Mountain hand and foot specimens resemble more closely those of the still undescribed Huang Mountain monkey (Tables 1 and 2).

DISCUSSION

The newly found Huang Mountain monkey is mainly ground-dwelling, according to sources at the Hefei Zoo. The body is large, about 70–90 cm in standing height. A tall individual could reach 1 meter. Its extremities are strongly built. It weighs more than 20 kilograms. A large male could weigh over 33 kilograms, while females would be smaller. The back hair is brown in color. The adult male has whiskers, and has a reddish color on the face. It is not active in disposition. It is mainly herbivorous, sometimes eating a little meat. It reportedly likes to eat bamboo shoots in the spring. These features are similar to those reported for the Jiulong Mountain animal killed in 1957.

The morphological features of the hands and feet of the Huang Mountain monkey are similar to those of the Jiulong Mountain specimens. Of special



FIG. 8.—The dermatoglyphic pattern of the right hand of *Macaca assamensis*.

interest is the fact that the Huang Mountain monkey's fingernails and toenails are flat, except for the "paw shape" of the second digits (Fig. 6). This feature is similar to the Jiulong Mountain animal, and is not found in other macaques. Dermatoglyphic patterns of the Huang Mountain monkey have also been obtained (Fig. 7), and may be compared to those of *Macaca assamensis* (Fig. 8).

Judging from the available data, we can conclude that the Jiulong Mountain "Manbear" killed in 1957 was a large stump-tailed macaque resembling the Huang Mountain monkey. It is still uncertain whether it represents a new species or subspecies.

It is noteworthy that a strange animal called "Huan" is recorded in *The History of Shuichang County*. It is reported that "Huan is like a monkey, big in size and black in color. Its disposition is slow." The discovery of the large Jiulong Mountain stump-tailed monkey, showing many similarities with the historical Huan, now provides strong scientific evidence in support of this ancient record.

It is also noteworthy that reports of a shorter Wildman, about 1.2 meters tall, have come from the western part of Yunnan Province and the Sheng-nongjia region in Hubei Province. It is the author's opinion that such Wildman reports result from sightings of these large monkeys. If this conclusion is correct, the discovery of the Huang Mountain monkey has important significance for solving the problem of the small Wildman in these areas. It should be noted that the Huang Mountain monkey appears to have been recorded in ancient local history with the same name, "Wildman."

I believe the hand and foot specimens of the Jiulong Mountain "Man bear" were not those of a Wildman, but belonged to a kind of large stump-tailed monkey unknown to science. It may well be the animal called "Huan" in ancient Chinese literature. Their population may be quite small, and they may even be extinct.

The identification of the Jiulong Mountain "Manbear" specimens does not solve the problem of Wildman in the Jiulong Mountain area. Wildman reportedly has a much larger body, over 2 meters tall, is fully bipedal, and leaves footprints about 30 cm in length. Further work will be necessary in order to solve the problem of Wildman in China.

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Field Reports

Field Reports are not reviewed by members of the Editorial Board of Cryptozoology or other outside referees. Reported descriptions or results of field work are the responsibility of the authors only, and are subject to criticism in the Comments and Responses section.

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ACTIVITIES OF THE ACADEMY OF APPLIED SCIENCE RELATED TO INVESTIGATIONS AT LOCH NESS, 1984

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INTRODUCTION

The question concerning the possible existence of large, unidentified animals in Loch Ness, Scotland, has been an unresolved one since at least the 1930's, when such animals began being reported on an international level. The Academy of Applied Science (AAS) began intensive operations at Loch Ness in the early 1970's. A summary of AAS research and results at the loch during the 1970's has appeared in this journal previously (see Robert H. Rines, 1982, Summarizing a Decade of Underwater Studies at Loch Ness, *Cryptozoology*, Vol. 1: 24-32).

This report updates the continuing testing of improved underwater camera equipment and techniques during the summer of 1984, preparatory to the planning of a future full-scale, extended-period, multi-team investigation. It is hoped that such investigation will ultimately produce definitive identification of the unknown residents of the loch. The 1984 fieldwork being reported on here took place at Loch Ness in July and early August.

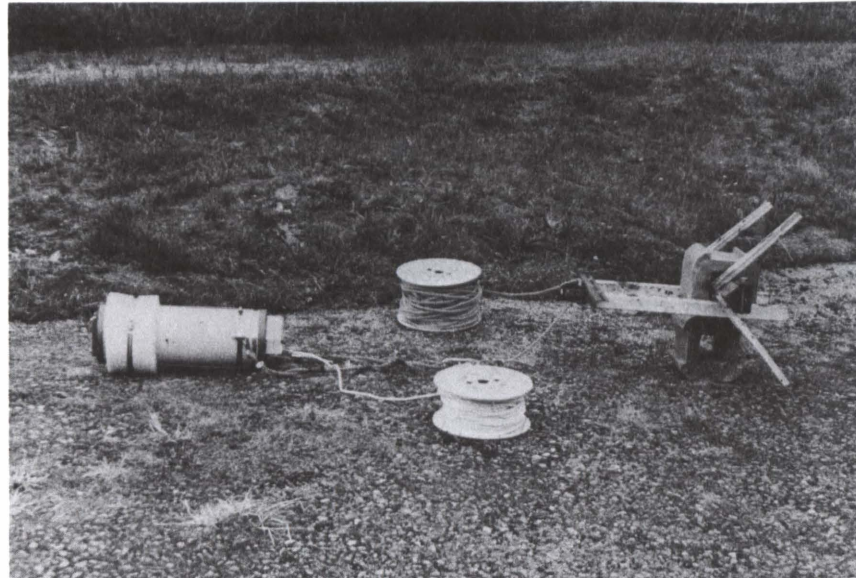


FIG. 1.—The unassembled parts of the silhouette camera and bottom mount.

NARRATIVE DESCRIPTION

Following on the most interesting earlier silhouette photographs obtained when the elapsed-time, upwardly pointed Edgerton 16mm camera of 1975 vintage continued exposing frames after the strobe batteries had been exhausted (see Robert H. Rines, Charles W. Wyckoff, Harold E. Edgerton, and Martin Klein, 1976, *Search for the Loch Ness Monster*, Figs. D and F, *Technology Review*, Vol. 78[5]: 25–40), Edgerton and his student Mark DeCew decided to test further the usefulness of this technique during daylight hours. The first tests were conducted in the Charles River across from the campus of the Massachusetts Institute of Technology. Actual on-site operations were conducted in Urquhart Bay, Loch Ness.

The camera and mount (Fig. 1) may be deployed in the loch in such a way that no suspending ropes or cables are required from the surface, thus leaving open space over the upwardly pointing camera, which floats submerged with tether passing through the camera mount (Fig. 2). With type 2484 film (2,000 16mm frames, f.2.8, with elapsed time periods of from 5 to 15 seconds), successful monitoring up to 7:00 p.m. during July was conducted at a depth of about 25 feet by DeCew and Robert Needleman. No significant target shadows were recorded.

It was found that scatter of daylight in the upper layers permitted some fill-in on calibration targets, so that detail other than just dark silhouettes



FIG. 2.—Simulation of the camera floating above the mount in a vertical orientation after deployment in the loch.

can sometimes be obtained. This may thus be a promising tool for future use, not requiring the complexity of electronic strobe and controls.

RESULTS

No photographic or electronic evidence of unknown animals was obtained during the 1984 fieldwork. As stated above, however, we are optimistic that the silhouette, elapsed-time camera technique tested further in 1984 may be useful over a sufficient number of daylight hours to be used as one of the monitoring tools for our future activities at Loch Ness.

FUTURE PLANS

We are starting the planning of what we hope may be definitive fieldwork. We invite suggestions on techniques and experiments that might be incorporated into what we expect to be a lengthy and more exhaustive investigation, perhaps one lasting year-round instead of just in the summer.

Up to the present time, our efforts have concentrated on what could be referred to as a "passive" mode; that is, the recording of whatever objects may pass into the range of our instruments. Changes in the future will include the "active" mode, in which our instruments will actually attempt to seek out whatever large animals may be inhabiting the loch.

A FIELD INVESTIGATION INTO THE RELICT HOMINOID SITUATION IN TAJIKISTAN, U.S.S.R.

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INTRODUCTION

From August 27 to September 22, 1982, the author visited three regions of Tajikistan for the purpose of learning first-hand the hominological situation there. (Hominology in the Soviet Union is the investigation of supposed unknown, human-like animals known as relict hominoids.) Tajikistan is a constituent Soviet republic in Soviet Central Asia, lying at the latitudes of northern California. Although it is a relatively small Soviet republic, comprising only 0.6 percent of all Soviet territory (Fig. 1), it is 55,237 square miles, about the size of Wisconsin, or Nepal, and somewhat bigger than Greece. Ninety-three percent of the republic's area consists of mountains, some of which are the highest in the U.S.S.R.

The beginnings of hominoid research in Tajikistan is connected with the name of Professor Boris Porshnev, who took part in a 1958 U.S.S.R. Academy of Sciences "Snowman" expedition, and who later visited the area several times. The Scientific Commission to Study the Snowman Question that existed at the time received accounts from General M. S. Topilsky, who claimed to have examined a slain hairy wildman in Tajikistan in 1925, from prospector B. M. Zdorik, who claimed to have stumbled on a sleeping hairy "dev" in 1934, from hydrologist A. G. Pronin, claiming a hominoid sighting in 1957, and some others.

When the lack of physical evidence resulted in the evaporating of official interest in the problem, Tajikistan (being several thousand kilometers from Moscow) took second place after the Caucasus in our self-funded hominology research. But in the late 1970's, factory worker and mountaineer Igor Tatsl, of Kiev, later supported by Igor Bourtsev, of Moscow, put much effort into bringing Tajikistan back to the fore of hominological activity. In 1980, two hominoid sightings were claimed by Tatsl expedition members, the first ever reported by actual field searchers. These developments, given publicity in the U.S.S.R. and abroad, met with enthusiastic acclaim from a segment of the public, and with silence or negative reaction from the scientific community. The Darwin Museum and newspaper editors received many pro and con letters from all over the U.S.S.R., including Tajikistan. Hence my decision to undertake a fact-finding trip to the area in 1982.

NARRATIVE DESCRIPTION

Upon arriving in Dushanbe, capital of Tajikistan, my colleague Vadim Makarov and I consulted local hominoid researchers, including geophysicist



FIG. 1.—Location of the Soviet Republic of Tajikistan, where the author conducted several weeks of fieldwork.

Karl Yefremov, who advised us to visit the region of Sary-Khosor, some 100 kilometers southeast of Dushanbe, where he had heard sighting accounts from the local population.

In the company of four associates from Dushanbe, Makarov and I trekked through the mountains for a week in the region of Sary-Khosor. Traveling along creek-beds and hardly discernible or very precipitous paths for several days, we encountered no human habitation. The fauna was most conspicuous by numerous and ever present bear tracks of different sizes, left mainly in creek-beds; I found this encouraging, as the bear and the supposed hominoid are about equal in weight and diet. The bear feces we found were full of stones from wild plums (*Prunus divaricata*) and undigested plums. Occasionally, we came across plum-trees with branches broken and bent down, and a lot of fruit strewn on the ground—leftovers of bear feasts. Bears (*Ursus arctos isabellinus*) are abundant in Tajikistan for two reasons: food is plentiful, and the Tajiks, being Moslems, do not eat bear meat. They trap bears only rarely, we were told, with nooses hidden in those plum-trees, to obtain bear fat, which is used for medicinal purposes.

In one place, toward the end of our mountain trek, we came upon a mud hut used by wild fruit and walnut pickers, a kind of seasonal dug-out in a gully slope. The hut was open, with no owner present. We could see pro-

visions inside the hut, and ubiquitous bear tracks not far outside. We then descended into the Shirop-dara valley, and met the first inhabitants. It was the summer camp of the wild fruit pickers whose "front-line" dug-out we had seen in the mountains. With traditional Tajik hospitality, they treated us to milk, honey, pancakes and tea. Conversation was difficult because the family spoke only poor Russian, and we had only a few Tajik words—mostly names of animals—at our command. Still, we understood that the grandmother of the family, who was present, had seen a *gul* (pronounced "gool," a common Tajik name for the supposed wildman), right in front of the hut in the mountains already known to us, when she was young. Details could not be obtained because of the language handicap. They said they knew of no recent sightings.

A Forest Service worker we interviewed later said he had seen the tracks of a *gul* when he was a boy. We had noticed the reluctance of most locals to discuss the subject, with the exception of educated Tajiks at the main Forest Service office in the village of Sary-Khosor. The director of the office, Saidov, and the village council chairman, Makhmat Buriev, said that from time to time they got information about wildman from hunters, shepherds, and Forest Service workers. In 1980, for example, they were surprised to see a flock of sheep driven from the mountain pastures to the village in mid-August (usually this is done in late September). An agitated shepherd told them that he had seen a big black *gul* near the pasture; his dogs had taken fright, and he did not have the courage to stay there any longer.

I asked whether *guls* are reported to steal sheep, and got a negative answer. However, shepherds sometimes report finding pelts of mice and gophers separated very nicely from the carcasses, and they believe the *gul* is responsible. Forked sticks are sometimes found near rodents' holes; these are supposed to be used by *guls* to catch mice.

The next leg of our journey took us to the Karatag gorge, in the Gissar Range, northwest of Dushanbe, the site of a purported sighting by two Tatsl expedition members at Lake Pairon in 1980. On the way to the lake, we met and talked with Gafar Jabirov, a local Forest Service patrolman, who claimed to have seen a wildman, and even fired at him, several years before, as was reported in the press by Igor Bourtsev. I found Jabirov to be a serious and respectable person, who had no doubt that he had seen a *gul* in the Karatag gorge, and I had no doubts concerning his sincerity.

We reached Lake Pairon, and inspected the site of expedition members Geliona Siforova's and Dima Sizov's purported sighting in September, 1980. Makarov and I were joined there by Igor Bourtsev and Geliona Siforova herself, who showed us the exact place and explained the details of the encounter. She and Sizov claim to have seen a female hominoid, 10 yards from where they were spending the night on the ground, under a tree on the shore of the mountain lake. The creature sat on a boulder watching them



FIG. 2.—Igor Tatsl's base camp by the Siama River, about 50 kilometers north of Dushanbe, capital of the Soviet Republic of Tajikistan. The base camp, in the Gissar Range, is located on the "crossroads" of two canyons, and is partially hidden by vegetation.

and making munching sounds for a long period of time. No footprints or hairs could be found in the morning where the creature had sat through its night vigil.

We spent several nights at the same spot, but no *guls* joined our company. On one night, I went and sat on the boulder, making various movements, while Bourtsev, lying 10 yards away, was telling (correctly) how I moved. We concluded that Siforova and Sizov could have at least made out the creature at that distance at night.

On the way back from the Karatag, I talked with a Forest Service man named Aslon Zakirov, at his office in Shakhrinai. He said he was much intrigued by the subject of wildman, and had interviewed his fellow-Tajiks on the matter whenever he had a chance. He told me of an encounter 5 years earlier of a Tajik hunter, whom he fully trusted, with "a giant hairy man, very broad in the shoulders, with the face like that of an ape." Zakirov

said that, though such encounters are very rare, Forest Service rules prohibit their employees from spending the night alone in the mountains for fear of these wildmen.

I then discussed the subject with an experienced local journalist in Dushanbe. For him, the existence of wildmen in Tajikistan was a matter of fact, but he doubted the problem could ever be solved without forceful official involvement. He wondered where and how the creatures spend their winters. The snow is two meters deep in the Gissar mountains in the winter. He said he had observed the area during helicopter flights, and could even spot mice tracks, but never those of a wildman. In his opinion, the hominoids migrate for the winter to the very south of Tajikistan, where there is little snow or none at all.

The third and last region I visited was on the Siama River, a tributary of the Varzob River, some 50 kilometers north of Dushanbe. This is the site of Igor Tatsl's activities in the last several years, where hominoid tracks and sightings have been claimed. I found the area good from a topological perspective; Tatsl's base camp stands on a "crossroads" of canyons, and is partially hidden by vegetation (Fig. 2), so that hominoids can supposedly safely approach the camp in their wanderings from one canyon to another. But how much of this really happens, and how much is just imagined by expedition members is impossible to tell at present. Nina Grinyova, who claimed a sighting in August, 1980, was present during my visit, and she re-enacted the scene for us. All I can conclude is that I found her story more believable there than I had in Moscow. Attempts to repeat her claimed achievement with similar tactics (staying alone at night) have proved fruitless so far. Food-baits and photo-traps have not worked, either.

RESULTS

Tajikistan appears to be an ecologically fitting habitat for relict hominoids. The abundant signs I witnessed of local fauna, particularly omnivores such as bears and wild pigs, indicate enough food resources for the presumably omnivorous hominoids the year round. Anthropogenic pressure on them is also no problem. The 93 percent of the republic's territory taken up by mountains is virtually devoid of permanent human population, so the latter poses no special danger to wild hominoids.

The long and continuing record of purported hominoid sightings in the area, supported by the new accounts discussed above, leads me to the conclusion that such creatures do exist there. But to turn this conclusion into a scientific fact is as difficult in Tajikistan as anywhere. Overoptimistic forecasts that appeared in the Soviet press in connection with Igor Tatsl's expeditions were based on wishful thinking, while some foreign reports exaggerated and distorted the situation out of all proportion.

As a result of this expedition, I am more inclined than ever to think that relict hominoids have neither "supernatural" nor simply "animal" status, but what I call "superanimal" status. And this is for quite natural reasons: their large brains, coupled with upright walking and free hands. These factors are responsible, perhaps, for their occasional strange behavior, on the one hand, and their "abnormal" elusiveness, on the other. The latter is largely achieved through what could be called a "guerrilla tactic of supermobility." In the words of a local hunter, the wildman "is as nimble-footed as a wild ram, as sharp-eyed as an eagle, and as good at hearing as a snow leopard. There is no one in the mountains to challenge him."

FUTURE PLANS

Tajikistan remains high on the agenda of our fieldwork. We aim to reduce the quantity and raise the quality of expedition members. One of our main concerns is to re-engage the scientific community in this investigation, and to raise funds for year-round fieldwork, instead of the two-three-month stints presently conducted at our own expense.

LCPI WORK AT LAKE CHAMPLAIN, 1984

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INTRODUCTION

In 1984, the Lake Champlain Phenomena Investigation (LCPI) conducted 17 days of on-site fieldwork at Lake Champlain in its ongoing investigations into the lake's supposed "Loch Ness-like animals," which are popularly dubbed "Champ." LCPI's fieldwork at the 109-mile-long lake was an abbreviated schedule compared to its 1982 effort (31 days) and its 1983 program (29 days).

The fieldwork was again oriented towards: (1) a continuation of daylight surface surveillance using cameras and binoculars; (2) further documentation and analysis of Champ sightings; (3) the encouragement of residents and visitors to Lake Champlain to carry cameras for photographic documentation of Champ sightings, and to report Champ sightings to LCPI; and (4) providing assistance to qualified and serious individuals and organizations in Champ-related research/fieldwork. New to our 1984 fieldwork was testing the use and application of a night-sight scope for nocturnal surface surveillance, which was donated to LCPI.

NARRATIVE DESCRIPTION

LCPI fieldwork in 1984 was conducted primarily by M. Pat Meaney and Joseph W. Zarzynski. Assistance during part of the fieldwork was provided by Rod Canham of NyPenn Divers, and ISC member Ted Straiton.

The emphasis of our fieldwork was shoreline and boat surface surveillance using several 35mm cameras with telephoto lenses, a super 8mm movie camera, camera tripods, a Venus Scientific Night Sight, and binoculars.

Lake surface surveillance was conducted at: Kimball Dock Pier, Vermont; Button Bay, Vermont; Bulwagga Bay, New York; and along other shoreline sites. Vessel surveillance was handled from the 13-foot-long Avon inflatable boat used in the past by LCPI.

Two scuba diving sessions were held as part of an underwater reconnaissance survey (June 26: Canham, Meaney, and Zarzynski; September 22: Meaney and Zarzynski).

The dates of the LCPI fieldwork sessions were: April 26, May 27, June 23-29, July 6, July 8, July 18, August 9, September 22-23, and September 29-30. Rod Canham assisted LCPI on June 26-27, and Ted Straiton assisted during a July 8 work session. On August 9, LCPI helped Richard Smith, of Wind & Whalebone Media Productions, during part of his 4-day underwater video work at Lake Champlain.

RESULTS

LCPI field operations in 1984 did not result in any visual sightings of unidentified animals by LCPI members or support personnel. Nor did scuba and sonar sessions note any possible Champ targets. Side scan sonar runs were done at the lake by two sonar teams working with the Champlain Maritime Society on underwater archaeological surveys. Neither team (one led by Jim Kennard, an LCPI associate, and the other conducted by John Fish) reported any sonar anomalies or targets that might be attributed to large unidentified animals.

However, 11 sightings of Champ were cataloged for 1984 by LCPI (Fig. 1), bringing the total number of reported Champ sightings to 235. A brief summary of the 11 sightings for 1984 is outlined below in chronological order:

- May 21, 1984: Anna Gagne; one-quarter mile south of Popasquash Island, near St. Albans, Vermont; 1 p.m.; sunny; a head and neck, but the eyewitness thought it could have been a loon.
- May 23, 1984: Gabriel Gagne, Anna Gagne, and Havaleh Gagne; observed southeast of Popasquash Island; 5 p.m.; rain; head and 3-4 feet of neck; dark gray, dark brown, or black in color; Gabriel Gagne saw the outline of its "fins" just above the surface of the water; plesiosaurus in appearance.
- July 5, 1984: Ronald C. Daly and Matt Cronin; in Shelburne Bay, Vermont; range 250-400 yards; 5:35 p.m.; weather warm; it had just stopped raining; lake surface smooth; head and three humps; color of animal was dark brown or black.
- July 12, 1984: Albert C. Spaulding; from a cottage located $\frac{7}{10}$ mile north of the Grand Isle-Cumberland Head ferry, on Grand Isle; range 200-300 yards; 10:30 a.m.; surface calm; black swan-like neck with a bulbous black head . . . not a wildfowl.
- July 18, 1984: Janet Hansen and two friends; Bulwagga Bay, Port Henry, New York; approximately 6 p.m.; while canoeing; range 150 yards; smooth lake surface; "head and long neck in one smooth arch."
- July 21, 1984: Norma J. Oppenlander and her husband; off Shelburne Farms Mansion, Shelburne Point, Vermont; while boating; 1:30 p.m.; very calm lake; brownish green in color; was undulating through the water north to south.
- July 28, 1984: William Togueville; about noon/1 p.m.; from his house in Rouses Point, New York; range 800 meters; two animals visible (six humps on first animal, five humps on second, with the animal's front hump a head); 40-50 feet long; animals black in color.
- July 28, 1984; Michael Shea, Bette Morris, and approximately 60 other

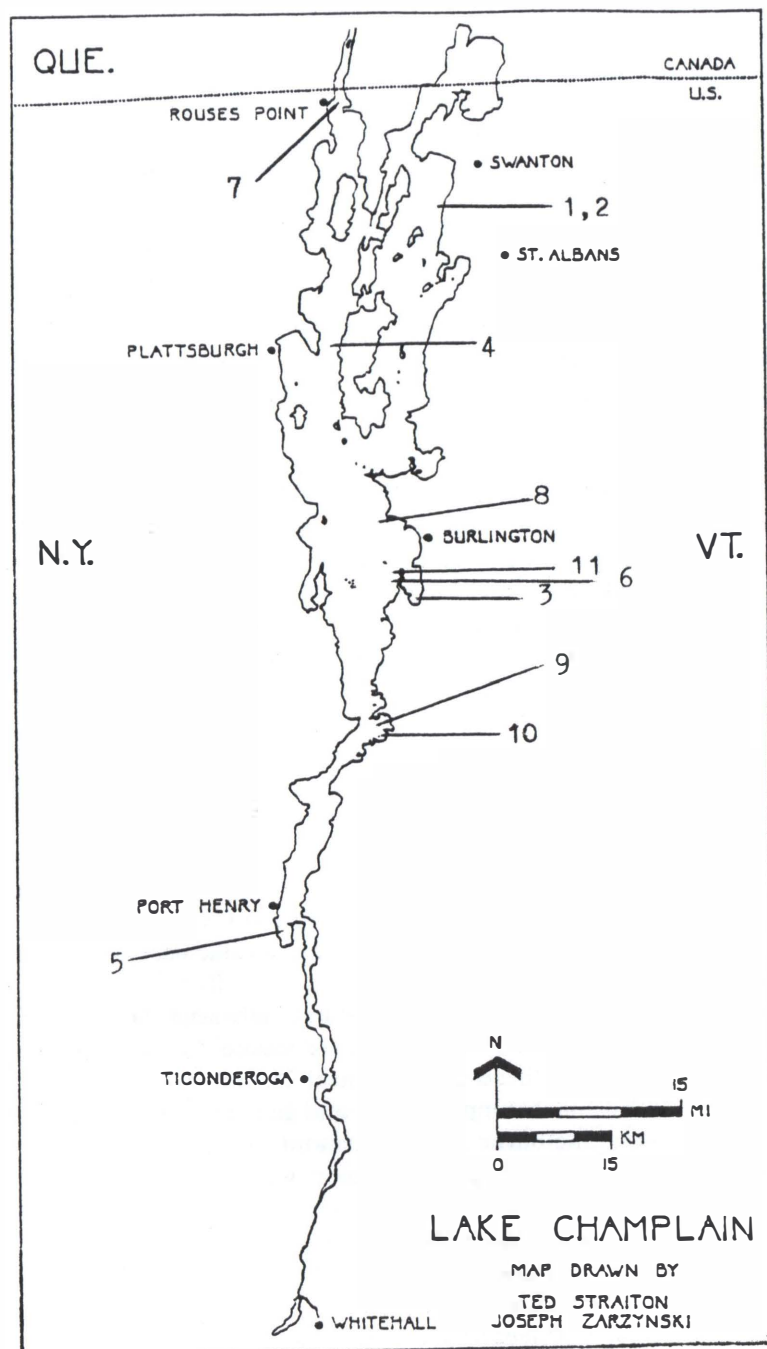


FIG. 1.— Map of Lake Champlain, with numbers indicating the locations of the 11 eyewitness sightings logged by LCPI during 1984.

people; approximately 6–6:30 p.m.; from the vessel *The Spirit of Ethan Allen*, just off Appletree Point, Burlington, Vermont; animal observed twice; range 200–300 feet; 30 feet long; dark, gray, or brown in color; Shea reported three–five humps, each about 15–20 inches tall; Morris wrote that she observed a head and three humps; reportedly a photograph or photographs were taken, but have not been published.

- July 27 or August 3, 1984: Eric Mills; off Ferrisburg, Vermont; in 80–100 feet of water; early afternoon; viewed for 10–15 minutes; 30 feet long.
- August 4, 1984; Eric Mills and his two sisters; from a boat “between Gardiner’s Island and the small island near shore at Long Point”; one large hump; hump was “grayish green and the sun glistened from it.”
- August 14, 1984; Jim Thurston, Jr., and several others; off Webb Estate, Shelburne Point, Vermont, towards Dunder Rock; from shore; range 100–150 yards; “brownish fish” color; 15 feet long with four humps; viewed through binoculars; was moving at about 5 miles per hour.

As in previous years, many of the sightings involved multiple witnesses, and the observation from *The Spirit of Ethan Allen* involved no fewer than 60 persons, surpassing the 35 witnesses who observed an unknown animal from a YMCA camp in 1983.

FUTURE PLANS

LCPI will continue its long-term field operations at Lake Champlain using similar strategies. In 1985, LCPI will encourage the Vermont Senate to pass the “Champ Resolution,” passed already by the Vermont House of Representatives, the New York State Senate, and the New York State Assembly (see Joseph W. Zarzynski, 1984, *Champ: Beyond the Legend*, Bannister Publications, Fort Henry, New York). Like fieldwork at Loch Ness, results at Lake Champlain are piecemeal, but they are important in piecing together this zoological jigsaw puzzle.

AN ATTEMPT TO OBTAIN A SPECIMEN OF SASQUATCH THROUGH PROLONGED FIELDWORK

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INTRODUCTION

Between May 19, 1984, and September 6, 1984, the author undertook fieldwork in the U.S. Pacific Northwest in an attempt to obtain a specimen of the supposed North American Sasquatch (Bigfoot). The exact location of the fieldwork has been kept confidential for several reasons: the publicity surrounding the expedition became a storm of controversy, with potentially serious legal implications; some hostile members of the public threatened to follow and disrupt the expedition; and I felt a responsibility to keep overzealous people from tramping through the target area, which would compromise the fieldwork, as well as harm the local environment and possibly the intruders as well.¹

That said, let me state that the method was to obtain, if possible, a Sasquatch specimen with a rifle. Photos are not considered proof, as has been demonstrated in the past. Also, as the physiology of the supposed animal is unknown, attempts at tranquilization would probably not be successful.

Approximately 12 years of prior but low-key interest in, and research on, the subject led to me undertake the endeavor and to finance it personally. My conviction that a real zoological problem existed crystallized when I discovered sets of unidentifiable tracks in 1980 in a remote area of the San Gabriel mountains in southern California.

A large body of physical, circumstantial, and anecdotal evidence indicates that such a creature as Sasquatch actually exists, can be collected, and therefore warrants an attempt to do so. It is clear such an attempt must involve a long-term commitment, and its success can only be through perseverance and determination.

¹ As a general editorial rule, manuscripts, whether they be literature reviews, laboratory analyses, or fieldwork, should contain all relevant details. Due to the circumstances of this particular case, however, an exception is being made. The circumstances involve possible legal action by official agencies in the state where the author conducted fieldwork, as well as disruptive action by private individuals. Keeping the location confidential was a condition set by the author, but in accepting this condition, the Editor in no way sanctions the author's methods of fieldwork or endorses any particular moral or legal position in the matter. The particulars of the fieldwork location are on file with the Society, and are available to any legitimate researchers with the permission of the author.—Editor.

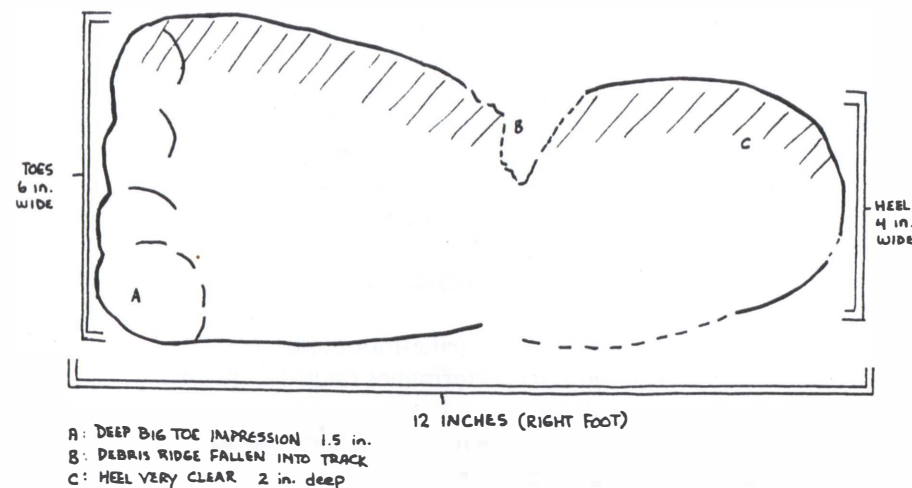


FIG. 1a.—Field sketch of the best of three tracks found on May 24, 1984. This 12-inch track was about 24 inches from the other two, which were only partial tracks.

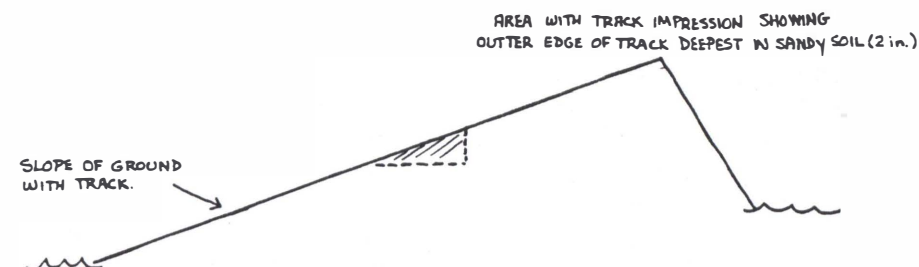


FIG. 1b.—Profile of ground where tracks were found. One partial track was behind and another in front of the full track appearing in Fig. 1a. Gravel and rock formed the ground except for one patch of sand upon which the full track was impressed. A creek flowed on either side of the gravel bar.

NARRATIVE DESCRIPTION

On May 19, 1984, my associate James Wyatt and I were dropped off in the snow on a back country road. A 10-hour hike took us into a valley targeted for the search. Surprisingly, the expected food resources thought to be available were not present, and near-starvation forced us to evacuate the area by a 17-hour trek on May 24.

We found three supposed Sasquatch tracks on our way out, which was heartening, but about which we could do nothing other than take photos and make field sketches. The second track, 12 inches in length, was the best of the three (Fig. 1a, 1b). It was very clear, and only about 24 hours old. The tracks were found on a gravel sand bar in a creek.

After reentry on June 4, 1984, we were subjected to near-freezing temperatures and constant rainfall, which lasted until the 9th. A shelter erected during that time was designated the base camp. It was of logs and brush; no tents or sleeping bags were used. Also, we depended entirely on an all-grain diet, rather than attempt to hunt legal game, as gunfire and blood scent could be counterproductive. We each subsisted on five handfuls of grain a day, with an occasional piece of carefully hoarded salami taken along for its fat content. Our weight allowance permitted us to pack in 3 weeks' worth of food at a time before resupply was necessary. Other equipment included the following three major items:

1) A Starlight night vision scope (rifle-mounted). This PVS-1 military model amplifies ambient light. Its performance ranged from poor to excellent depending on the light available.

2) Several PSID sensors: these are military seismic sensors used for monitoring movement near the base camp.

3) A .338 Winchester magnum rifle, which I feel is of sufficient caliber to bring down a large ape.

Bear (*Ursus americanus*) were encountered often. Our first bear attack occurred on June 9; it resulted from mutual surprise, but turned out well for all. The only other disagreeable encounter was when one stalked me to within 15 feet, at which point it fled when confronted.

A single, good supposed Sasquatch track, 13 inches long, was found in a boggy area on June 17. Fieldwork continued until June 23, the resupply date at which time we hiked out of the valley. As we sat on the roadside waiting, I decided to gather wood to make a small beacon for the expected resupply driver. It was then that we had the most exciting find of the expedition. A large scuff mark between two trees caused me to look further, and I found a series of four good bipedal tracks approximately one hour old. Circumstances indicated that the animal was on the road, heard our approach, and hurriedly fled the area. No other evidence was found at the site. Photos were taken and drawings were made (Fig. 2). The tracks were about 13 inches long, and closely resembled those of May 24. The weight of the animal was estimated at 500 lbs. The time of their deposition was estimated at 7 p.m.

Upon our return to Eureka, California, I was arrested by the Humboldt County Sheriff's Department for possession of an infrared sniper scope. I had taken it to a gunshop to have the mount repaired. The alleged violation was of California Penal Code, Sec. 468, which prohibits the use of infrared sniper scopes except by law enforcement agencies. After 4 days, however, the charges were dropped, the scope was returned, and it was admitted that I possessed only a Starlight scope, which is quite legal.

During that period, Robert Moore replaced James Wyatt as my field associate, although he was only available for 3 weeks. Requisite self-discipline was paramount in any field participant, due to the harsh conditions

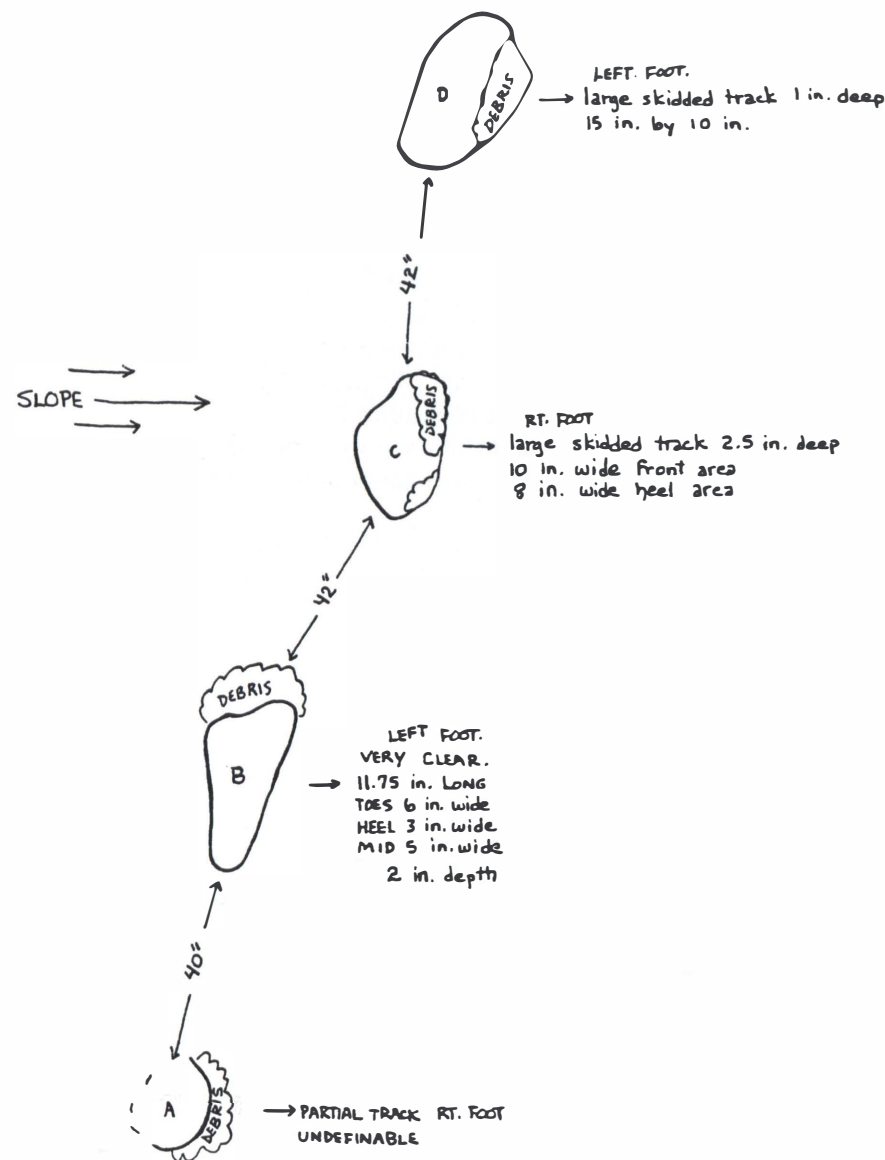


FIG. 2.—Field sketch of four tracks found on June 23, 1984. The tracks were deposited on hard, rocky terrain, and disappeared into rocks and brush. The bipedal animal that made them was probably moving quickly, though not at a run.

of the fieldwork. We not only stalked through rough and difficult terrain, but we were often required to lie immobile for hours in rocks and timber while we observed areas of interest. This was often the case at night also, so sleep was obtained only sporadically.

In July, a scat was found with two sets of tracks (big and small). I estimated them to be about 10 days old. The fecal sample was sent to the International Society of Cryptozoology for analysis, as was a fossil claw found in a stream bed.

No evidence of any note was discovered in August.

As to other animal life encountered, we saw an abundance of bear, deer, grouse, gray squirrels, and mice. Flora comprised first-growth Douglas fir, huge cedars, aspen, and huckleberries. We also encountered patches of a relic plant known commonly as "Cobra lilies."

In no part of the target area did we find any evidence whatsoever of human passage, and, indeed, the nearest human habitation was about 70 miles from us.

The last segment of the expedition I conducted alone, as other participants were not available. By September my funds were exhausted, and the fieldwork was terminated, although I had hoped to continue into mid-October.

RESULTS

The fieldwork resulted in five sets of tracks, most of which consisted of one full track and two or three partials. The ground was very rocky and strewn with forest debris, and tracks of any nature were difficult to locate. The scat is probably too old for enzymal analysis, though dietary information may be extracted, as well as, hopefully, parasitic information. The sample is currently being analyzed.

Other results are a more complete logistical understanding of the requirements for low-profile hunting of this supposed type of animal, with the supposition that the area is deserving of extended scrutiny; three such animals appeared to be using the area.

FUTURE PLANS

The author intends to undertake another expedition into this area and an adjoining area in 1985. Funding acquired over the next months will determine whether it will be conducted. Should a Sasquatch specimen eventually be acquired, it will be made available for appropriate scientific study.

TESTING AN UNDERWATER VIDEO SYSTEM AT LAKE CHAMPLAIN

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INTRODUCTION

In August, 1984, Wind & Whalebone Media Productions field-tested at Lake Champlain an integrated method of obtaining video images of, and other data related to, large, unidentified aquatic animals reported in the lake and said to be "Loch Ness-like animals" (see Joseph W. Zarzynski, 1982, LCPI Work at Lake Champlain: 1982, *Cryptozoology*, Vol. 1: 73-77; and 1983, LCPI Work at Lake Champlain: 1983, *Cryptozoology*, Vol. 2: 126-131).

It is the author's opinion that the most successful work in this field has involved studious review of available eyewitness sighting data, with a goal to predicting the animals' behavior patterns, and/or deployment of underwater probes, especially camera equipment (see Robert H. Rines, 1982, Summarizing a Decade of Underwater Studies at Loch Ness, *Cryptozoology*, Vol. 1: 24-32). The least successful efforts seem to have been hampered by an approach to the lakes as two-dimensional surfaces of water from which unidentified animals may make random appearances, such as is done with surface surveillance from camera set-ups on shore.

Few investigators, if any, seem to have specifically approached the problem in terms of the three-dimensional nature of the animals' habitat; that is, thinking of the lakes as "drowned valleys" with certain bottom contour features that may be favored by the animals for feeding, resting, and other behavior. A number of sonar contacts at or near lake bottom obtained by various expeditions at Loch Ness, and consistent eyewitness accounts of these animals sinking straight down (as opposed to diving forward), circumstantially indicate that they may spend a significant amount of time lying on the bottom. The identification, location, and investigation of such features in the lake basin or "valley," and the introduction of camera, sonar, or other probes into them might increase the probability of contacts with such animals.

Use of a moving lure of fish, their probable food source, in conjunction with the probe might further enhance this strategy. Dick Pool, of Spokane, Washington, for example, has successfully obtained footage of bait-striking behavior in Pacific salmon using various lures trawled behind an underwater video camera. (See Dwight Schuh, 1982, Salmon on TV, *Outdoor Life* [April], pp. 56-67, 118.) I was unaware of Pool's work while first developing my ideas, and thank Joe Zarzynski for bringing this article to my attention.

The intent of the 1984 work at Lake Champlain was: (1) to test an underwater video system deployed from a small boat, using the camera in both vertical, bottom inspection mode, and a horizontal, trawling mode; (2) to test the use of a lure of fish in conjunction with the trawling mode; and (3) to concentrate on submerged portions of the lake basin in the vicinity of confirmed surface sightings whose features (based on available bathymetric data) might be congenial to such animals (such features could be ledges or depressions for bottom resting behavior, or ridges along which fish might gather).

Wind & Whalebone Media fieldwork at Champlain in 1984 consisted of 14 days on site, with 4 days of actual field tests. Exercises were coordinated by the author. Assistance was given by Wendy Lathrop, Dennis and Janet Hall, Fred Keller, A. Burger, Joe Zarzynski, Paul Crane, and Deanna Parsons. In addition, Joan Crane, John Durant, Ed Parsons, Chris Stokes and Jane Garland provided logistical support.

The interest and suggestions of experienced researchers in this field were much appreciated. Joe Zarzynski, director of the Lake Champlain Phenomena Investigation (LCPI) was most helpful and supportive during the planning period, and he assisted during the first day of field trials. Ike Blonder, a board member of the Academy of Applied Science (AAS), visited our operations with his wife Lois. Mr. Blonder kindly provided a hydrophone for our use. Howard and Ruth Curtis, also of AAS, stopped in the area as well.

Grateful acknowledgment is also made of the support of Robert Durant, Margaret Light, and Mae W. Smith of this season's work.

NARRATIVE DESCRIPTION

Primary testing was conducted on a Hydro Products TC-125 underwater black-and-white video camera with supplemental LT-7 underwater light. The camera was deployed from a 22-foot double outboard motor boat. Video images were received in the boat on a black-and-white monitor and simultaneously recorded on a half-inch VHS portable video recorder (Fig. 1). The camera was housed inside a trawling wing designed by David Olsen.

Although surface watching was not emphasized during our activities, still cameras and binoculars were carried by the author and some other group members when on or near the lake in the event of a sighting. A 35mm camera with 300mm lens was the author's camera of choice during peak daylight hours. Other 35mm and Instamatic-type cameras with wide-angle lenses were used for general documentation of expedition activities.

Operations were carried out near a base camp at Essex, New York. This area was chosen for ease of operations, basin features of interest, past history of sightings, and water clarity.

August 1 through 8 were spent in camp set-up and final arrangements,

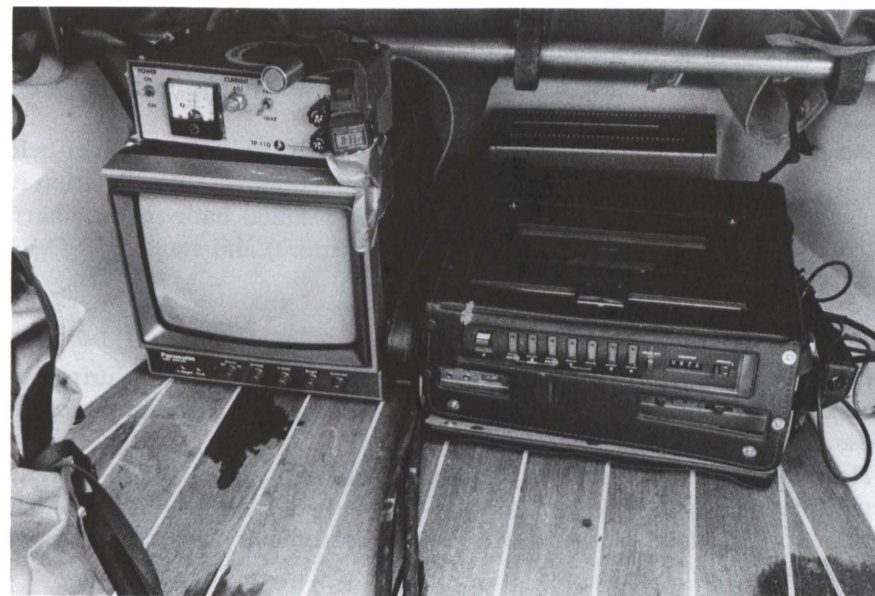


FIG. 1.—Video recorder and monitor in bow of boat during operations. Eel used as target lure was visible during this time in lower left of monitor, although photography of the screen in daylight renders it indistinct.

including site selections. August 9 was devoted first to simple deployment of the underwater video camera from the boat. The camera was trawled at the surface at a depth of approximately 12 feet, without supplemental light in the horizontal mode, trailing a simple aluminum can 4 feet behind the camera as a target. Next, the camera was deployed in a vertical scan mode, with light at 70 feet. The lake bottom was observed and recorded. These activities were carried out in Whallon Bay. August 10 involved deployment of camera with light and lure of two dead eels trawled behind the moving camera (eels were chosen for tests for their availability, their inexpensiveness, and also to avoid any conflicts that might arise with the use of game fish in this manner). The camera/lure was deployed off Split Rock Point in about 300 feet of water. The camera/lure was played out 250 feet from the boat and trawled at a depth of 140 feet. A similar test was carried out in the same location with a single eel target at 150 feet on August 11. August 12 saw a test of camera deployment along a submerged ridge off Bluff's Point, starting at 60 feet and moving down to 175 feet. Eel lure and supplemental light were used throughout this trial.

Field trials were carried out in the afternoon, except for August 12, which was an early-morning (5–9 a.m.) effort. All night work planned for August 11 was canceled due to threatening weather.

Hydrophone recording was carried out on all four camera test days. The maximum depth of the hydrophone line used was 63 feet. Water temperature measurements were made throughout, but the simple "fisherman's thermometer" used was probably not very accurate.

An attempt was made to use a fish-finder sonar unit and a diver-held sonar gun in depth measurements, but both proved inoperative. Position in the lake basin was estimated on the basis of detailed bathymetric charts (provided by Allen S. Hunt, University of Vermont) and standard NOAA navigational charts for the area.

RESULTS

No evidence of large, unidentified lake animals was recorded during our time at Lake Champlain, nor were there any sightings during that period by those working on the field tests.

However, the camera system performed well in its first deployment at an actual site. There were no breakdowns of video equipment. With practice, the camera/lure was deployed with only minimal difficulty from the relatively small boat.

Although the eel lure performed adequately, the targets moving with an approximation of swimming motion, much work needs to be done in refining the lure so as to present a target attractive to the animals being sought. Experienced anglers will readily attest to the fact that a lure seemingly realistic to the fisher may not attract the interest of the fish—or if it does, it may not do so all the time. In theory, the notion of inducing on-camera target pursuit and feeding behavior among large, unidentified lake animals has fascinating potential, but the gap between potential and reality is still large.

There was no direct observation on the video of any fish; this was a disappointment, as it was hoped to share any footage of fish swimming attitudes with professional ichthyologists with whom the author had consulted during planning stages. As the amount of actual in-water camera time averaged only 2 hours daily, however, there was little realistic chance for such events this first season. Also, some prime evening time was canceled due to weather conditions as noted, which decreased chances for observations of both known and unknown aquatic animals.

Overall, most of the goals set for this initial on-site test of the underwater video system and method were fulfilled, and this first season was deemed a success.

FUTURE PLANS

Wind & Whalebone Media Productions will return to Lake Champlain in future seasons with additional and refined equipment. Research will proceed along the lines discussed above. Operations may be held in another part or parts of the lake with a greater recorded incidence of Champ sightings.

Liaisons will be maintained and expanded with professional scientists in

regard to these efforts. Public education will be carried out via selected press contacts, but with a strict eye towards maintaining the integrity of the research, and cordial, low-key community relations.

CRYPTOZOOLOGY: THE SEARCH FOR UNKNOWN OR SUPPOSEDLY EXTINCT ANIMALS

July 7, 1985

a symposium of the

III International Congress of Systematic and Evolutionary Biology (ICSEB III)

July 4-10, 1985

University of Sussex
Brighton, England



Morning Programme

Chairman's Introductory Remarks

David Heppell, Department of Natural History, Royal Scottish Museum, Edinburgh, Scotland, U.K.

"Splitting versus Lumping in Systematic Zoology and Cryptozoology"

Bernard Heuvelmans, Centre for Cryptozoology, Le Bugue, France

"A Classificatory System for Cryptozoology"

J. Richard Greenwell, International Society of Cryptozoology, Tucson, Arizona, U.S.A.

"Linguistics as a Tool in Cryptozoological Research"

Piotr Klafkowski, Solberg, Norway

Afternoon Programme

"Histological and Amino Acid Analyses of *Octopus Giganteus* Tissue"

Joseph F. Gennaro, Jr., Department of Biology, New York University, New York, New York, U.S.A., and Roy P. Mackal, Office of the President, The University of Chicago, Chicago, Illinois, U.S.A.

"Fossil Ungulates in the Archaeological Record"

Christine M. Janis, Division of Biology and Medicine, Brown University, Providence, Rhode Island, U.S.A.

"The Onza as a Paleo-Cheetah: An Example of Possible Pleistocene Persistence"

Helmut Hemmer, Department of Zoology, Johannes Gutenberg University of Mainz, Mainz, Federal German Republic

"A Species Named from Footprints"

Grover S. Krantz, Department of Anthropology, Washington State University, Pullman, Washington, U.S.A.

Panel Discussion and Questions from the Floor

The morning programme will begin at 9:00 a.m. and end at 11:30 a.m.

The afternoon programme will begin at 1:00 p.m. and end at 5:00 p.m.

For additional information, contact the International Society of Cryptozoology, P.O. Box 43070, Tucson, Arizona 85733, U.S.A.

For registrations, contact the ICSEB III Congress Office, 130 Queens Road, Brighton, Sussex BN1 3WE, U.K.

Sponsors: The Royal Society, the Linnean Society, the Paleontological Association, the Systematics Association, the British Ecological Society, and the International Union of Biological Sciences.

Book Reviews

Cryptozoology, 3, 1984, 94–108
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The Guinness Book of Animal Facts & Feats. By Gerald L. Wood. Guinness Superlatives, Enfield, Middlesex, England, 1982. 252 pp. £8.95 (c.).

The first paragraph of the author's introduction succinctly summarizes what this work is about:

In this book an attempt has been made to provide the answers to a vast number of interesting questions about animal extremes—answers which the average reader cannot find by consulting generally-available literature—and in the process expose many of the half-truths and exaggerated statements that are synonymous with this fascinating subject.

It might be noted that “answers which the average reader cannot find by consulting generally-available literature” is a considerable understatement. Gerald Wood has not only assiduously searched the zoological literature, he has communicated with dozens of scientists, zookeepers, and other authoritative sources of esoteric zoological information. He also acknowledges the “hundreds of other correspondents” who provided material for this third, completely updated edition.

The book is divided into 14 sections, beginning with Mammals, progressing through the other classes of vertebrates (Birds, Reptiles, Amphibians, Fishes) and the major groups of invertebrates, and ending with Prehistoric Animals. Within each section are accounts, varying in length from a few lines to half-a-dozen pages, dealing with the various superlatives with which the author is concerned—largest, smallest, fastest, longest lived, most dangerous, rarest, deepest diving, etc., etc.

The longer entries include much additional information. For example, in the account of “The Largest Living Bird”—the ostrich—one finds heights and weights of notable specimens; geographic ranges of the subspecies and how those differ from one another; behavior of the cock ostrich, “one of the most dangerous animals on earth” during the mating season; the basis for the old belief that an ostrich will bury its head in the sand when frightened; the trainability of ostriches (they have been trained to herd sheep and chase off foraging birds); and observations of their unexpected swimming ability.

Readers with a cryptozoological bent will find items of special interest. For example, there is a detailed explanation of why the decomposed remains of basking sharks are often identified as “sea serpents.” Megamouth, the 14½-foot shark accidentally caught by a Navy research vessel off Oahu in

1976, is discussed. This specimen was the sole representative of an entirely new family of sharks until a second one, of the same size, was taken in a fisherman's gill net off the coast of California late in 1984.

There is a lengthy entry on giant squids, and another on giant octopuses. The latter includes an account of *Octopus giganteus*, the name given to huge remains that washed ashore at St. Augustine Beach, Florida, in 1896. Also mentioned here is a 30-foot long “tremendously bulky monster with enormous eyes,” which was found on a beach in Scotland in 1944.

The last section, Prehistoric Animals, contains much interesting information on animals which were the largest of their kind ever to inhabit the earth. Through correspondence with paleontologists, Wood learned of fossil remains indicating the past existence of dinosaurs far larger than those commonly considered to be among the biggest. Other records included in this section are for the largest known flying creature (a pterosaur, or flying reptile, with an estimated wing span of 36–39 feet), the largest known crocodile (53 feet long), and the largest known turtle (estimated to have weighed 4,500 pounds), to name only a few.

The book is not totally error-free, but given its scope and the tremendous amount of specific detail it contains, no one could reasonably expect it to be. The very few errors my particular luggage of zoological knowledge enabled me to spot were of a relatively minor nature—for example, an incorrect scientific name for a particular sea turtle and a photograph of a porcupine fish captioned: “The death puffer, the most poisonous fish known to science.” In view of Wood's meticulous documentation of his facts, his inclusion of scientific names (common names are often unreliable for identification), and the numerous indications that he has taken pains to present the most reliable information available, I suspect that any errors came from his sources.

The book is, to use the conventional expression, profusely illustrated, not only with black and white photos but also many color plates. On the endpapers inside the front cover is a table, “Animal Velocity,” listing probable maximum speeds for dozens of species. Inside the back cover is a similar table titled “Animal Longevity.” There is a bibliography containing over 900 entries and a comprehensive index.

The Guinness Book of Animal Facts & Feats can be dipped into anywhere for pleasure and edification, aside from its value as a unique reference work. I recommend it without reservation.

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(Abstracts from this book appear regularly in the “Wood's Animal Facts” column in *The ISC Newsletter*—Editor.)

On the Track of the Mystery Animal: The Story of the Discovery of the Okapi.
By Miriam Schlein. Four Winds Press, New York, 1978. 58 pp. \$6.95 (c.).

The widely respected children's book author of this engaging, well-written, and accurate recounting of the discovery of the okapi (the animal adopted as the logo of the International Society of Cryptozoology), never actually uses the term "cryptozoology." Nevertheless, the elements of a classical cryptozoological detective story flow through the book and are reflected in the persistent efforts of Sir Harry Johnston, the man responsible for its discovery.

For anyone interested in introducing youngsters (ages 9–16) to the excitement of the search for mystery animals, this book is a fine place to start. Ruth Sanderson's pen-and-ink drawings throughout the book are charming; also included are Johnston's own excellent illustrations of the okapi.

The story opens during the 19th Century, with the unknown okapi isolated from centers of science in the Central African tropical rain forest. But here the pygmies knew it well, and had named it *okwapi*. It was as a child in London in the 1860's that Johnston took his first step toward the discovery of the okapi when he read a book on new animals. In it, it was suggested that a rumored horse-like animal with horns from the African rain forest might be the fanciful unicorn. The possibility caught the young Johnston's imagination, and it lingered in his mind even after he had grown to become an accomplished artist, talented linguist, and African colonial administrator. Then, in 1888, he read a report of a "donkey" known to the pygmies of Central Africa, and he wondered if there was a connection between the two reports, one relating it to a known animal and the other relating it to a mythical one. Johnston saw the incongruity of a member of the horse family living in dense jungle, and he resolved to pursue the question. Luck took him directly to the pygmies.

The pygmies reported to him that the animal had a foot with more than one hoof. Thus, Johnston knew it could not be a horse, at least not a modern horse. He became excited by the idea that it might be a living survivor of *Hipparion*, the extinct three-toed horse. Searching for the animal in the Congo, he was only able to obtain pieces of striped zebra-like skin from soldiers. When the pygmies showed him the two-toed, cloven tracks of the animal in the forest, he reacted with disbelief, thinking they were misleading him. These tracks matched neither of his expectations, neither the single-toed track of a present day horse, zebra, or ass, nor the three-toed track of *Hipparion*. Malaria abruptly terminated the search, but Johnston left word of his interest in obtaining a full skin, skull, and hooves.

The author then diverges slightly from the story to discuss the problems presented to scientists by the pieces of skin that Johnston had sent to London, noting the characteristics of the orders Perissodactyla, the odd-toed hoofed

mammals, and Artiodactyla, the even-toed hoofed mammals. His discovery was tentatively classified as *Equus johnstoni*, a new horse-like animal.

Shortly after the description of this new animal from the partial skin, Johnston, still in Africa, received two skulls and a full skin with attached two-toed hooves. Now he knew his mystery animal could not be in the horse family; it was an artiodactyl, not a perissodactyl. The tracks the pygmies had shown him in the Congo really *had* belonged to it. The skull provided especially valuable clues, having bilobed lower canine teeth and "horn-cores," both characteristics of the giraffe family. All the evidence from down the years was finally fitting together. The animal was a member of the giraffes, with only one other living species. As the author relates, Johnston sent his new specimens to the British Museum (Natural History), with the suggestion that the animal might be a living survivor of a fossil giraffe, *Helladotherium*, an animal he had read about. This idea proved incorrect. The already named new animal was now placed in its own new genus, *Okapia*, in the family Giraffidae. The author ends by discussing the life of the okapi as we know this forest dweller today.

Perhaps no other cryptozoological tale contains so many elements of a mystery story—and so satisfactory a conclusion. A large, unknown animal, suggested by native reports, was at first conceptualized in terms of both real and mythical creatures. The search for its discovery encompassed a lifetime of persistent efforts by a man whose first inspiration was in childhood, whose first reaction to real tracks was disbelief, and whose first conclusions, based on partial evidence (skins), were incorrect. Even after more conclusive evidence was available (hooves and skulls), it took scientific evaluation before a clear understanding emerged concerning the okapi's evolutionary relationships. The importance of specimens in the ultimate resolution of cryptozoological problems is clear, as is the determination of individuals willing to follow the sometimes tortuous paths to truth.

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Loch Ness Monster. By Tim Dinsdale. Routledge & Kegan Paul, London, 1982. 218 pp. \$9.50 (p.).

Loch Ness Monster was first published in 1961, and has since appeared in three revised and updated editions: the second in 1972, the third in 1976,

and the fourth, reviewed here, in 1982. In reviewing this book, it will be necessary to consider the work as standing alone for the first-time reader, and from the viewpoint of those familiar with the earlier editions. For this latter purpose, a comparison with the first edition will be most instructive, since the other two editions were simply transitional forms, comparisons with which time and space do not permit.

The book represents Tim Dinsdale's personal view and experience investigating the mystery of Loch Ness. Dinsdale is the Loch Ness Monster investigator *par excellence*, having spent a great portion of his life—since his sighting and filming of the monster in 1960—attempting to prove the existence and nature of the phenomenon in the loch. Consequently, his views and evidential experience deserve consideration, and they must be taken into account in any serious analysis of this subject.

As information accumulated after 1961 at an ever-increasing rate, an updated version would be expected to grow greatly in size. This is not the case. Dinsdale has deleted sufficient early material so that new data could be added without increasing the length of the text. The procedure is not always helpful, however, as some of the deleted material is important, especially from an historical perspective. The evolution of Dinsdale's viewpoint over the years is reflected in his personal comments about some of the evidence, which can be compared in each edition. I found such a comparison very rewarding, since changing one's opinion in the face of new evidence is perfectly legitimate and laudable.

No change in Dinsdale's favorite identification of the alleged animals in Loch Ness as plesiosaurs has taken place. To his credit, as a non-zoologist, he writes, "Its identification I consider a matter for experts in zoology." This modest position is reflected in the reduction of the discussion of the possible nature of Loch Ness animals to only a little more than four pages from more than a chapter in the first edition. A further improvement in the book is the elimination of material related to other lake and sea "monsters," which was far too cursory and mostly tangential in earlier editions. An appendix, containing much valuable data, has been added.

The first seven chapters in both editions are almost identical, even as far as chapter headings are concerned. Some rewriting is apparent, reflecting slight changes in viewpoint, updating, and the like. These chapters cover mainly Dinsdale's own involvement with the Loch Ness Monster, and document his own efforts to solve the mystery. This is fascinating and instructive reading for everyone concerned with Loch Ness and what it may contain.

Chapter eight, although now entitled "Review of Interesting Cases" instead of "Special Evidence," departs little from the earlier format. Chapter nine is a brief historical review of activities at Loch Ness between 1960 and 1972, confined mostly to events in which Dinsdale himself was involved, including another brief sighting of the phenomenon. Lastly, Chapter ten, only four

pages and a bit, deals very briefly with the identification of what is being observed, photographed, and sonar-contacted.

Appendix A continues the report of activities at Loch Ness over the time span 1972 through June, 1981. Appendix B lists later sonar results (1972–78), plus a reference to the 1980 use of a newly designed Academy of Applied Science sonar target trigger system, with inconclusive results. Appendix C is devoted exclusively to the Shiels photographs and the Smith film. One of the two Shiels photographs also adorns the front cover of the book. It should be noted that the Shiels photographs are controversial, a large segment of informed opinion holding that the pictures are fakes. Analysis and discussion of these items is included.

In Appendix D, Dinsdale reevaluates once again some of the more classic "monster" photographs, such as the Gray photograph, the surgeon's (Wilson) photograph, Lachlan Stuart's photograph, the MacNab photograph, the Academy of Applied Science 1975 underwater photography and, once more, the 1977 Shiels photographs and the Smith film, finally concluding with a discussion of sonar results and some material about Loch Morar sightings. In the last two paragraphs, entitled "The Future," Dinsdale states that he is writing a sequel to one of his other books, *Project Water Horse*, entitled *Loch Ness and the Water Unicorn*.

Loch Ness Monster, in its fourth edition, is better than earlier editions in some respects and worse in others. It continues to be an interesting account—although very far from comprehensive—and it certainly is required reading for anyone concerned with the Loch Ness phenomenon.

The book is written with a totally positive bias, with no critical comments to speak of. One wonders what has happened to all the frauds, hoaxes, and mistaken identities which constitute possibly 90 per cent of the anecdotal and photographic evidence. I suspect that only those items which Dinsdale himself believes in have been reported. Regarding everything else (for example, the Peter O'Connor and Frank Searle photography, just to mention two), there is silence. If a certain book length is to be maintained, however, selectivity is required, and coverage inevitably will be spotty and uneven. And perhaps, in the end, silence is more damaging than denunciation.

Perhaps the most annoying negative feature of this new edition is the deletion of the index. To serious students, this omission is almost, if not quite, unforgivable. On the positive side, many of the sketches and drawings have been redone in a much more professional manner. I found the most engaging aspect of the book to be the "hands on," personal style of certainly the most dedicated Loch Ness Monster hunter of all time.

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Monstres des Lacs du Québec: Mythes et Troublantes Réalités [Monsters in Quebec Lakes: Myths and Troublesome Realities]. By Michel Meurger and Claude Gagnon. Stanké, Montréal, 1982. 320 pp. \$12.95 (Canadian) (p.).

Too many myths, not enough reality. Cryptozoology, like any other field of science, will progress only through careful, systematic, and objective observations and descriptions.

This book is nevertheless informative because it gives a first look at many reports of unexplained sightings of "lake monsters" in Canada's Province of Quebec. Throughout this vast area, there are many coinciding testimonies relating the presence in the waters, on the shores, and in the nearby swamps of unknown animals of large size. The authors also consider watershed networks with large lakes where sightings have been reported. This approach in synthesis is quite useful, and more work in that direction should be encouraged.

There are a number of well-documented sightings in Quebec, which leads one to accept the existence of large, unknown, aquatic animals. These testimonies came from two sources: (1) contemporary, ordinary, Anglo eyewitnesses who simply relate what they see. The independent comparison of these reports is most interesting: certain key features of these cryptic animals come back again and again; and (2) reports from Amerindian eyewitnesses which are most informative. Here, we have accounts by people with no scientific prejudice, nor belief in one theory or another. Only one thing counts for these people—survival.

Too often, anthropologists use whatever information suits them, and attribute the rest to superstition, magic, and the like. In my opinion, however, there is an enigma to be faced, and not transformed into imaginary concepts. I believe in the axiom: "Myths and legends are the memory of people."

One difficulty with this book lies in its poor organization; all kinds of topics more or less related are presented with a lot of repetition. Local facts are presented together with those from Europe and elsewhere. An account of divers finding an underwater stone wall in Lake Memphremagog is relevant to archaeology, no cryptozoology.

The major weaknesses in the book, however, are too many errors, incorrect assumptions, unsubstantiated feelings, and not enough facts. There are too many cases of "bottomless lakes," black waters, swirling currents, underwater tunnels between lakes and/or between lakes and the nearby ocean, too many accounts of unrecovered bodies of drowned people, etc. Finally, the author lost me with his discussion of "paranormal vision," not because I don't believe in it, but because it confuses the reader in this context.

Cryptozoology requires the collaboration of researchers from many areas working together and with a truly scientific approach. The limnological and/

or geomorphological uncertainties displayed in this book do more harm than good in elucidating the question of cryptic animals.

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Monsters Among Us. By Brad Steiger. Para Research, Rockport, Massachusetts, 1982. 166 pp. \$9.95 (p.).

At the end of the first chapter, author Brad Steiger lets us know what sort of book this is to be:

I cannot guarantee that this book will make you believe in monsters, but I am quite confident that I can guarantee a few good chills up the spine, some dandy tingles in the solar plexus and more than one cautious look over your shoulder. You may even go so far as to check beneath your bed before you crawl under the covers.

So this is not to be a book of cryptozoology, or even a book of folklore and legend; it is to be a book of scary stories culled from a variety of sources. It would be churlish, as well as pointless, to review the book as anything else. To be sure, there is a bit of theorizing about the elusiveness of monsters as part of a "Reality Game" played with "undeclared cosmic opponents." It's the sort of speculation that college sophomores engage in after a few beers, and the broad-minded Steiger even tells the reader to come up with his own theory if he wishes. Fortunately, there is not much of this fluff, and it does not seriously interfere with the main purpose of the book, which is to tell a scary story.

The author must be judged on his skill as a storyteller, and as a collector of odd and unusual accounts.

In this age when 12-year-olds gleefully attend horror films, and the blood-and-vomit epics of Stephen King regularly top the best-seller lists, there is absolutely nothing in this book that could "chill the spine" of the average, or even the unusually sensitive reader. Some portions may not be suitable for small children and maiden aunts, but that's about it. For example, some of the activities attributed to Elisabeth Bathory, the so-called vampire countess, are a bit gruesome, but they jump out at one primarily because most of the book is so bland.

Readers who have more than a casual acquaintance with cryptozoology and the folklore of monsters will find little new here. There were a few items that were new to me, but since Steiger only fitfully gives his sources, it's

impossible to know whether these are bits of genuine folklore, or something that Steiger or someone else simply made up on the spur of the moment.

The two roughly cryptozoological chapters, "Giant Reptiles, Sea Monsters and Lake Longnecks" and "The Abominable Snowman and Wildman Around the World," are probably the worst in the book. These stories simply are not very scary anyway, and the author's attempt to make them so, while maintaining their aura of "scientific truth," is self-defeating and irritating. The "expert" most frequently cited is Ivan T. Sanderson, and from that one may judge for oneself the reliability of the reports.

Steiger is better when he gets to vampires and werewolves because he is dealing with better stories, and he can throw away all pretense of science and get down to the main business of entertaining the reader.

In a book like this, I'm willing to accept a pretty broad definition of "monster," but Elisabeth Bathory more properly belongs in a book on magic and witchcraft. The story of the phantom hitchhiker is ghost lore, and Burke and Hare are completely out of place; perhaps in a book on crime, or even the history of medicine, but monsters? Never! If one is going to cast one's net that widely, why not Charles Manson or Adolph Hitler? I suppose one must fill the pages with something.

To sum up, *Monsters Among Us* is an average "gee whiz" book. Readers interested in this sort of material should go back to the original sources, people like Sanderson, Montague Summers, and John Keel. They were no more reliable, but they were more original and had genuinely eccentric minds. Also, they just did it better.

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The Encyclopedia of Monsters. By Daniel Cohen. Dodd, Mead, New York, 1982. 287 pp. \$14.95 (c.).

In the September, 1972, issue of the journal *Limnology and Oceanography*, R. W. Sheldon and S. R. Kerr, of Canada's Bedford Institute of Oceanography, estimated the population density of monsters in Loch Ness. In thanking an associate for having brought the matter to their attention, they stated that they had been previously "unaware that monsters were a problem."

Monsters are still a problem, mainly because we are never quite sure what we really mean when we say "monster." A monster may be any large, unknown (or merely unidentified) marine animal, a long-necked lake animal,

any unknown terrestrial animal of unusual size, or a large human-like creature such as Bigfoot. It can also include various kinds of mythical or folkloric entities, from classical Greek demigods to eastern European vampires. The unifying thread that seems to run through all monsters is a subjective and unstated supposition of grotesqueness and malevolence—although with some, like Nessie, this is less obvious (in fact, affection for these animals has improved their reputation). Thus, the concept of "monster" has little to do with a creature's form or its classificatory status within the animal kingdom. Consequently, some monsters are cryptozoological, and some are not; likewise, some cryptozoological animals can be labeled "monsters," and others really cannot or should not be.

All of these problems are reflected in Daniel Cohen's *Encyclopedia of Monsters*, which provides 103 descriptive capsules on monsters, as well as on other living, extinct, or "supposed" animals which this reviewer does *not* consider to be "monstrous." Of the 103 descriptions, only 59 (61 percent) can be considered directly or indirectly cryptozoological. (Most of these are to be found in the chapters entitled "Humanoids," "Land Monsters," "River and Lake Monsters," and "Sea Monsters." Other sections deal more with folkloric and supernatural creatures.) This brings us back again to the problem surrounding the concept of "monster," especially when one uses the term in the title of a book, and even more so in that of an encyclopedia. Quite a few of the animals included, such as the okapi, the thylacine, the moa, and the coelacanth, can hardly be considered monsters (curiously, the Komodo dragon—a true cryptozoological monster of its time—is not included).

Cohen himself anticipates some of these criticisms by stating in his introduction that he "had to expand and bend [the] definition a bit . . . I have also found it necessary to include information on several creatures that are neither unknown nor particularly monstrous in any way, but whose histories have had an influence on the study of monsters . . . The selection of the items in this book is an entirely personal one. I tried to stick to certain general criteria, but in the end I relied on an instinct developed through a lifelong interest in monsters and nearly 20 years of professional involvement with them." Indeed, Cohen, a former managing editor of *Science Digest*, is the author of more than 100 books, most of them dealing directly or indirectly with monsters or the bizarre.

The following comments may be made on specific sections. Those on sea and lake monsters, the Yeti, Bigfoot, and Wildman are fairly straightforward and historically accurate. We visit the Iceman once more, and Cohen takes a very negative position (which will again frustrate Bernard Heuvelmans, who continually reminds Americans that they criticize without reading his book on the topic): "[Heuvelmans and Ivan T. Sanderson] were suckered into endorsing a carnival promoter's model as a genuine unknown animal."

He also states that "both Sanderson and Heuvelmans backed off from their earlier support of the Iceman" after a hoax supposedly became apparent. I spent a weekend with Sanderson shortly before his death in the early 1970's, and I do not recall him "backing off" at all, and Heuvelmans certainly has not done so to this day.

In discussing the Beast of Truro, which represents one of a series of reports of supposed eastern cougars (*Felis concolor*), also known in the West as pumas or mountain lions, Cohen doubts this possibility because "there are no mountains in Cape Cod [Massachusetts], much less mountain lions," although he ponders over the possibility of "one" having survived—survived from when or where is not made clear.

A number of factual errors may also be found: The Mayan culture (p. 43) did *not* extend to South America. Professor Roy Frieden (p. 121) is at the University of Arizona, *not* Arizona State University (which I believe is some sort of remote and obscure institution elsewhere in Arizona!). The Royal Air Force's Joint Air Reconnaissance Intelligence Center, when studying the 1960 Dinsdale film of Nessie (p. 129), concluded that the hump was between 12 and 16 feet in length, *not* "as much as ninety feet long." Crocodiles are found in marine environments, not just rivers and swamps (p. 171).

While this review has perhaps focused on the shortcomings of the book, I do not wish to give a wrong impression of my overall opinion of it. It is generally a well-researched, well-written, and, above all, enjoyable volume, as Cohen's works tend to be. It never becomes dull, vague, or cynical, and it has the right touch of humor. Although unrelated to cryptozoology, some of the better parts of the book are in the final chapter on creatures of folklore, particularly the sections on werewolves and vampires. Here Cohen is at his best, having covered this material so many times before.

The volume has a modest annotated bibliography, but fortunately includes the eccentric works of the early 20th Century English curate Montague Summers, who held delightfully medieval beliefs in vampires and werewolves. But that's another story.

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The Bigfoot Casebook. By Janet and Colin Bord. Stackpole Books, Harrisburg, Pennsylvania, 1982. 254 pp. \$10.95 (p.).

The Bords' book, particularly pp. 151–230, will be a welcome addition to the libraries of all seeking information about American Sasquatches north of Mexico. These pages consist, for the years 1818–1980, inclusive, of a

chronologic catalogue of all sightings of Sasquatches for which a record exists; the entries number slightly more than a thousand. If a few refer to actual humans, demented in the woods (as obviously true of a few, particularly the earlier ones), the total will be replenished by cases of sightings of actual Sasquatches not yet reported when the book went to press.

For each entry in the chronological list, the following essential data are presented: date, location, identity of observer, abstract of the description of the event, and place or places of publication of the description by the observer or a reporter (newspaper, book, or newsletter). A few of the entries have all of the available information packed into one line, most run to three or four, and a few have six, seven, or even eight lines.

The larger part (two-thirds) of the book consists of longer descriptions of those sightings which the authors regarded as most interesting. While they have omitted some of the oft-told tales (Jacko of British Columbia, for instance), they undoubtedly included some accounts not usually found in earlier anthologies (I have not cross-checked all such collections).

I emphasize that *The Bigfoot Casebook* lists only sightings, and does not cover other topics except as those may be mentioned as peripheral to a sighting. Thus, footprints, usually a major theme of books on Sasquatches, are here omitted unless directly associated with sightings. For almost all other aspects of evolution, ecology, anatomy, etc., the Bords simply refer the reader to other sources, which they list in a bibliography. A useful index is also included.

The Bords have obviously been concerned with several cryptic problems other than Sasquatches, and are willing to consider paranormal explanations on occasion for such problems. Such tendencies on their part, however, are here minimally expressed, and they do not intrude into the usefulness of the book.

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A World-Shocking Theft [in Chinese]. By Zhou Guoxing. Chinese Academy of Sciences, Henan Publishing House of Science and Technology, Beijing, 1982. 188 pp. n/p. (p.).

This small popular science book includes about two dozen essays on anthropological subjects. The title of the book is that of one of these essays, which relates the disappearance of the original fossils of Peking Man (*Homo erectus pekinensis*) during World War II, and the current efforts by some

Chinese and Americans to discover their possible current whereabouts. Only the first group of essays, on "The Mystery of the Wildmen," is of direct relevance to cryptozoology, and will be reviewed here in some detail. The other essays are nevertheless of interest for the glimpse which they afford of popular scientific literature in modern China; they are a blend of modern and traditional, of worldly and folksy.

Among the essays are articles on biological variation in humans, always a fascinating subject, exemplified here by instances of excessive mammary development and hairiness in a young woman from Zhejiang, and by the opposite observation of total hairlessness in the province of Yunan. These anomalies are discussed in the light of archaeological evidence—the Zhejiang woman is compared to a steatopygic Stone Age female statuette—and of possible atavistic effects.

Recent developments in the theory of human origins are expounded in a series of brief essays which describe fossil apes and their discoveries all over the world. There is, in particular, a description of hominoid fossils found from the 1950's to the 1980's in Yunan, accompanied by a color photograph of a *Ramapithecus* mandible discovered in that province.

One of the more traditional topics discussed is "Qigong," a classical Chinese way of making people strong, both physically and mentally. Qigong, like Yoga, focuses on inner strengths, and is said to improve the functioning of internal organs and of the nervous and endocrine systems.

The cryptozoological essays, on the theme "The Mystery of the Wildmen," are founded on eyewitness reports of human-like, giant creatures accumulated over the past several decades. These reports have caused a great deal of interest, and the Chinese Academy of Sciences set up a research team to study them. According to witnesses, most Wildmen which have been seen are huge and hair-covered, with features intermediate between those of humans and apes. There are reports that some Wildmen were killed or captured. Two witnesses are quoted as particularly reliable. One, a biologist, saw a dead female, two meters tall, covered with grey-brown body hair, with large breasts and a face reminiscent of Peking Man (one of the book's plates shows an artistic reconstruction of the head of a female *Homo erectus* sculpted under the direction of the author). The other is a geologist who observed two Wildmen from a short distance; thought to be mother and child, they bore a strong resemblance to humans. The smaller one was about 1.6 m tall.

The Chinese expedition to Tibet in 1959 brought back a strand of hair which, though demonstrably neither orang-utan, nor yak, nor bear, could not be attributed to the Snowman which they were seeking. In the 1960's, the story shifts to the province of Yunan, where highway workers are said to have encountered and killed a Wildman about 1.3 m tall, covered with black hair, and which walked erect and possessed human-like hands, ears, breasts and external genitals. No material remains were preserved. In the

following decade, numerous eyewitness reports from the central provinces of Hubei and Shaanxi (whereas Yunan is in the south, just north of Burma, these two provinces are in central China, about 1,000 km inland of Shanghai) referred to some kind of huge Wildmen with long, red body hair, features intermediate between those of men and apes, and which walked erect, leaving large footprints in their wake. In 1977, a large-scale scientific expedition was launched to probe this mystery; the search involved more than 100 participants, who searched primeval forests for these supposed creatures. Unfortunately, only traces were found: footprints, hair (a strand of which, found in the Shennongjia forest, is shown in a color photograph), and feces. The search goes on. More recently, another investigation was prompted by a report that a farmer from the province of Zhejiang (on the coast, just north of Taiwan) had killed a "Manbear" some years back, in 1957, and that the hands and feet thereof had been preserved by a biology teacher. A color photo is shown in the book. Readers of *Cryptozoology* are already familiar with the hands, a picture of which was reproduced in Vol. I. The author is of the opinion that these limbs belong to an unknown species of large monkey.¹

What are these mysterious Wildmen? While the author admits that the evidence is fragmentary, and that many opinions prevail, he does not think that they belong to the human lineage. To him, Wildman may be some unknown primate of the primeval forest. Perhaps it is related to large, early apes, whose descendants are today also reported in North America under the name Bigfoot or Sasquatch. Thus, the wheel comes full circle and brings together cryptozoologists from separate continents and cultures faced with similar puzzles.

The author's name is already familiar to cryptozoologists as a member of the Board of Directors of the International Society of Cryptozoology. A short biography is appended to the book, and gives some details about Zhou's life. Well known for his popular scientific essays on the origins of man, Zhou's interest in anthropology arose early in his life. Born in the city of Nantong, on the Yangtze River, he was attracted as a boy to the natural history museum in his home-town. In high school, he read a book on Peking Man, written by Professor Pei Wen-Zhong, and he then decided to devote his life to anthropology. His studies at Fudan University ranged beyond his major subject to geosciences, philosophy, psychology, and sociology. After graduation, he worked at the Institute of Vertebrate Paleontology and Paleoanthropology of the Chinese Academy of Sciences, and was deeply involved in the interpretation of fossil teeth discovered in Yuanmou, in the province of Yunan. He now works at the Beijing Natural History Museum.

¹ See Zhou's Research Report elsewhere in this issue—Editor.

His research has been characterized by his extensive travel throughout China, compiling a "Photo-album of Paleohumans in China," and discovering the remains of microlithic cultures in Henan and Yunan provinces. His keen interest in current as well as paleo-mysteries accounts for his involvement in cryptozoology, and for his participation in numerous expeditions in search of Wildman.

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The Editor regrets several errors in the Comment by Leigh M. Van Valen, Cryptozoology, Vol. 2: 155-57.

Comments and Responses

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This section permits readers to critique or comment on works previously published in Cryptozoology. The original authors and other readers are encouraged to respond to these critiques or comments. Readers are also encouraged to critique or comment on the works appearing in this issue. All comments are the responsibility of the authors only, and do not reflect any policies established by the Editor or the Editorial Board of Cryptozoology, or the Board of Directors of the Society.

CRYPTOZOOLOGY: WHAT IT REALLY IS

(Response to Heppell and Van Valen, *Cryptozoology*, Vol. 2: 147-57)

Nothing could delight me more than David Heppell's critical but most judicious remarks on the sections of my article "What Is Cryptozoology?" devoted to the use of parataxa, and to the alleged requirement of a type specimen for the naming of a new species. It is obvious that I am not quite familiar with all the subtleties of the International Code of Zoological Nomenclature. This, incidentally, is understandable, if not excusable, for a zoologist, since these arbitrary, complex, and sometimes ambiguous rules belong more to the realm of law than to the realm of science.

However, while it is true, as Heppell specifies it, that a type specimen is not required before a taxon can be named, I certainly never stated that "a complete specimen" is required. On the contrary, I spoke (erroneously, I must admit) of the requirement of a type specimen "even if constituted by a simple anatomical fragment (as is sometimes the case in paleontology)."

As a matter of fact, the perfectly documented precisions given by Heppell, which I have been hoping for for a long time, comfort me immensely, as they confirm the validity of all the scientific names I have been bold enough to give in the past to some unrecorded species as a result of thorough cryptozoological analyses. Not only *Homo pongoides*, which was represented by a type specimen, but also *Dinanthropoides nivalis* for the Himalayan Yeti, and the five names of the best-documented types of "sea serpents," including *Megalotaria longicollis* for the long-necked type, which can crawl on land and often enter fjords or lakes in cold and temperate climates, i.e., Loch Ness in Scotland. Incidentally, *Megalotaria longicollis* antedates by about

10 years *Nessiteras rhombopteryx*, which is thought by many people to be the "official" name of the unknown animals inhabiting Loch Ness. I want Heppell to know how grateful I am to him.

Van Valen's criticisms are also generally on the mark (although his own belief in the need for a type specimen is also contradicted by Heppell's restatements). I even suspect that he takes the part of the devil's advocate to make the case for cryptozoology that much stronger.

All the same, I really do not think that the use of the name "cryptozoa" in a narrow and realistic sense makes the more suggestive name "cryptozoology" ambiguous and thus unfortunate. The great majority of people, even among zoologists, have never heard of cryptozoa. Who will ever take it that cryptozoology is restricted to their sole study? It is not because the name "Mesozoa" is used for a most obscure phylum of minute parasites that one should be tempted to believe that the Mesozoic era refers to the period during which these animalcules flourished.

Another point, but of importance: to me, Van Valen is wrong when he states that scientific nomenclature is inappropriate "for mermaids or jinn." (And so is Sibert, incidentally, in his Comment on Wagner, when he says that he has yet to hear of "cryptozoologists searching for leprechauns.") Mermaids, jinn, and the like are nothing else but the highly mythicized images of real animals (in the broadest sense). Mermaids are just distorted traditions of sirenians, pinnipeds, and sometimes dolphins, but possibly also of a still unknown form of aquatic primate (this, at least, cannot be ruled out completely). The name *jinn* was used by Mohammed the Prophet himself for wild hairy men living in the deserts, and which, he said, were not to be killed or even harmed, but, on the contrary, should be fed by the faithful. These scavenging outcasts were probably, in my opinion, relict Neanderthals. Finally, I know of several serious studies which try to link leprechauns, brownies, fairies, and tuttiquanti to the traditions of extinct prehistoric races (or species?) of pygmies.

Cryptozoologists should read more carefully what I said about the snares of mythification in "What Is Cryptozoology?," or, even better, in my recent article in *Fortean Times* (No. 41, Winter, 1983), "On Monsters: Or How Unknown Animals Become Fabulous Animals."

Van Valen also states that my definition of cryptozoology (the study of "animals undescribed by science") is "worse" than the one which would restrict it to the study of "animals of folklore," because *all* unknown species would then become the object of cryptozoological research. This is obviously a misunderstanding. It has been clearly emphasized in both my articles in *Cryptozoology* (Vols. 1 and 2)—Van Valen had probably not read the latter yet—that cryptozoology is the search for animal species still undescribed by science *but on which some information is available*, and should thus not be confused with the traditional method of blind zoological collection, mostly

performed at random. By giving several specific examples of cryptozoological achievements, my historical review in the present issue of *Cryptozoology* should help to clear up the last ambiguities and obscurities of what cryptozoology really is, has been, and should ideally be.

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BUT HOW MANY LARGE, TERRESTRIAL ANIMAL SPECIES REMAIN TO BE DISCOVERED?

(Comment on Heuvelmans, *Cryptozoology*, Vol. 2: 1–24)

Heuvelmans makes the point that new animal species are being discovered all the time. This is quite true; but the question needs to be refined a little. Leaving aside the ocean vastness, where the point that new species of large size have very recently been discovered—and more may be expected—is readily appreciated, all the species of interest to cryptozoologists are in fact large and terrestrial. So, how many animals that are large and live on land are undiscovered? How many mammals?

Each year sees the description of a few new species of small mammals. Bats, rodents, shrews, and dasyurid marsupials have all acquired recruits in the last decade or less. But these, like the hundreds of insects that are discovered each year, are hardly the Yeti or Nandi Bear. Among the larger mammals, the number added to the list is far smaller; over the past decade, I can report only six:

1) *Catagonus wagneri*, a peccary described originally as a fossil, but discovered to be still living by Wetzel in 1975, with a very small range in the Chaco, on the Paraguay/Brazil/Argentina border (see Ralph M. Wetzel, 1977, The Chacoan Peccary *Catagonus wagneri* [Rusconi], *Bulletin of the Carnegie Museum of Natural History*, Vol. 3: 1–36). Although its existence was an open secret for years among the local hunters, its external resemblance to the collared peccary, *Tayassu tajacu*, may have hindered its discovery by zoologists.

2) *Cercopithecus salongo* Thys van den Audenaerde, 1977, a monkey still known only by a single incomplete skin, although there seems no doubt of its distinctness as a species (see Dirk F. E. Thys van den Audenaerde, 1977, Description of a Monkey-Skin From East-Central Zaire as a Probably New Monkey-Species [Mammalia, Cercopithecidae], *Revue de Zoologie Afri-*

caine, Vol. 91: 1000–10). It lives somewhere in central Zaire, but its range must be very limited as it is not reported as part of the fauna in several study sites for such primates as the pygmy chimpanzee or the black mangabey.

3) *Potorous longipes* Seebeck & Johnston, 1980, the largest of the family Potoroidae ("rat-kangaroos"), inhabiting a limited area in Victoria (see J. H. Seebeck and P. G. Johnston, 1980, *Potorous longipes* [Marsupialia: Macropodidae], a New Species from Eastern Victoria, *Australian Journal of Zoology*, Vol. 28: 119–34). Its range is still incompletely known, as the area is very densely vegetated; besides which, it is probable that most Victorians are not aware even of the common potoroo, *Potorous tridactylus*, which is superficially similar.

4) *Moschus fuscus* Li, 1981, a species of musk deer described from the mountains of western Yunnan Province, China (see Li Zhixiang, 1981, On a New Species of Musk-Deer from China, *Zoological Research*, Vol. 2: 157–61 [Chinese, English Summary]). Three species inhabit the same area; the new species is distinguished by its very dark color.

5) *Petrogale persephone* Maynes, 1982, a species of rock wallaby which, unlike the other new species, is highly distinctive in its external characteristics (see G. M. Maynes, 1982, A New Species of Rock Wallaby, *Petrogale persephone* [Marsupialia: Macropodidae], from Proserpine, Central Queensland, *Australian Mammalogy*, Vol. 5: 47–58). It lives near a fair-sized population center (Proserpine, in Queensland, Australia), but its range is extremely restricted.

6) *Muntiacus atherodes* Groves & Grubb, 1982, a muntjak deer found all over the island of Borneo (see Colin P. Groves and Peter Grubb, 1982, The Species of Muntjak [Genus *Muntiacus*] in Borneo: Unrecognized Sympatry in Tropical Deer, *Zoologische Mededelingen [Leiden]*, Vol. 56: 203–16). It simply had not previously been differentiated from the larger, darker *M. muntjak* with which it shares its range.

This list is likely to be enlarged in the near future. I know of a new gazelle, and at least one new species of monkey which will be described shortly, and, of course, we may see a new bear if a recent report is confirmed (see *The ISC Newsletter*, Vol. 3[1]). A slender list, but we can make some observations on it. The monkey and the musk deer live in regions of sparse populations, rarely visited by scientists or by knowledgeable explorers. The peccary, the potoroo, and the muntjak are not very distinctive externally, and could easily be mistaken for something else—indeed, in the case of the muntjak, we know that it was continually thought to be merely young specimens of a related species. The rock wallaby, and also the potoroo, live in areas whose local inhabitants, though they pass through the area often enough, are not (or were not, until recently), particularly aware of wildlife; but it must be ad-

mitted that the case of *Petrogale persephone* is exceptional because it is so different in appearance from its relatives.

So, where may we expect new, large terrestrial mammals to turn up? I think the above list suggests that there are two potential sources.

The first is in museums. My colleague and I "discovered" *Muntiacus atherodes* by examining specimens and realizing that there were two consistently different kinds of muntjak on Borneo. *Moschus fuscus* was discovered on a field trip, but only a zoologist who knew what he was doing could have discovered it. *Potorous longipes* was brought to light after examination of a road-kill. A glance at skulls of peccaries killed by hunters in the Chaco revealed that *Catagonus wagneri* was still in the land of the living. In fact, specimens of both the "new" peccary and the "new" musk deer turned out to have been in museums all along, but nobody had bothered to look in the cabinets for undescribed species.

The second source would be unknown areas of the globe; but again, it would seem that professional zoologists, or people familiar with local faunas, would have the best chance of making such discoveries. A Japanese primatologist working on pygmy chimpanzees once bought a monkey skin from a hunter, and knew enough to identify it at least as "odd," and to send it to the Central African Museum in Tervuren, Belgium, where it could be compared with abundant other skins from Zaire. Who but the late Li Zhixiang would have known that the musk deer that had just slipped into the shadows was something unusual?

It is perhaps no surprise that central Zaire and western Yunnan have yielded new species of large mammals recently. It was in central Zaire that another monkey, *Cercopithecus dryas*, was described by Schwarz in the early 1930's (see E. Schwarz, 1932, Der Vertreter der Diana-Meerkatze in Zentral-Afrika, *Revue de Zoologie et de Botanique Africaines*, Vol. 21: 251–54). It is still known only by the type specimen. Other large mammals of the region are also poorly known, not least the pygmy chimpanzee, *Pan paniscus*, which hopeful field-workers have had great difficulty in finding.

As for western Yunnan, this is the center of a region which seems to be zoogeographically rather special, but is very poorly known. In the early 1960's, Hayman described a new species of goat antelope, the Red Goral, *Nemorhaedus cranbrookii*, from the mountains of northern Burma (see R. W. Hayman, 1961, The Red Goral of the Northeast Frontier Region, *Proceedings of the Zoological Society of London*, Vol. 136: 317–24). He noted its similarity to another species described 48 years earlier from southeastern Tibet, *Nemorhaedus baileyi*, and known only from the type specimen. Recent surveys of the western Yunnan mountains, lying between the type localities of these two goral, have found more red goral, linking *cranbrookii* with *baileyi* and making it fairly clear that they are, in fact, the same. Another rare

munthjak, *Muntiacus feai*, known previously from a very few specimens from Burma, has recently turned up in the same western Yunnan/southeastern Tibet region. One of the largest of the monkeys, the black snub-nosed monkey (*Pygathrix* [*Rhinopithecus*] *bieti*), has recently been rediscovered in the same region after being unrecorded since its first description nearly 90 years ago.

So what price cryptozoology? I think that future discoveries of new large species on the land will mostly be relatively unexciting, and will be the product of diligent taxonomic study rather than of exploration: black musk deer are hardly as stunning and as showy as kouprey, and cryptozoology badly needs another kouprey to be taken really seriously. (Perhaps the International Society of Cryptozoology should take as its motto: "Forty years since the kouprey is too long!") But there exists places where such things might yet turn up.

I suggest the cryptozoologist needs to err on the side of caution. Much of the cited evidence for the Yeti and the like really does have a commonplace explanation, and, so far, the zoological exploration of potential Yeti country has turned up only commonplace animals; new species, to be sure, but things only a zoologist or an ardent nature lover could get excited about. Tibetans, the other Chinese "nationalities," African tribespeople—they are not children of nature any more than are the good burghers of Proserpine who failed to recognize a new species of rock wallaby that lived on their very doorstep.

I well remember a field trip I took part in, to the lower Tana River, Kenya, in 1981. We asked the local people—two tribes, Pokomo and Orma—what animal made the loud wails we were hearing at night. Many people did not know; many obviously did not wish to know; most were scared stiff of the bush and all its denizens, and wished heartily that the elephants, buffaloes, and whatever made those loud wails would go away and leave their crops and their cattle and chickens in peace. Well, the wails were made by bushbabies, by hyraxes, and by the occasional hyena—animals that were known by sight to at least some people, *but simply not associated with those particular vocalizations*. It is an irresistible hypothesis that a couple of sightings of rarely seen, but not immediately identifiable animals, plus the vocalizations of a couple more (quite different!) animals, could add up to a Nandi Bear.

So let us, by all means, ask: "Is there such a thing as a Yeti?"; but let us not fail to ask the other questions: "What animals, known to science but perhaps rarely seen even where they live, have in the past been identified as a Yeti?" and "Where in this world could totally new, large, terrestrial species still turn up?" The questions are important. We may, at the same time, be putting our fingers on a conservation issue as well. What areas of habitat are undisturbed enough to shelter a large distinctive species? What animals are seen rarely enough to explain Yeti sightings? I have a terrible feeling that, as with the kouprey, there is a last-in/first-out principle involved;

if a *really distinctive* species has remained unknown this long, it is because it is very rare. If a real creature exists up in them thar hills—a distinctive creature that is the backbone of the Yeti legend, even if not the basis for *all* the Yeti stories—there would seem to be every danger that its eventual discovery may precede its extinction by only five or 10 years.

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CRYPTOZOOLOGY IS AFTER BOTH AQUATIC AND TERRESTRIAL ANIMALS, NOT TO MENTION MISINFORMED ZOOLOGISTS

(Response to Groves)

One problem with cryptozoology is that zoologists who endeavor to minimize the importance of its ends and means, and—I must add—many self-styled cryptozoologists, are grossly misinformed about its real scope. I am currently completing a *Checklist of Unknown Animals Cryptozoology Is Concerned With*, which, when published, should clear up several ambiguities and misunderstandings. Groves, for example, would certainly not state that "all the species of interest to cryptozoology are in fact large and terrestrial." Nor would the late George Gaylord Simpson, in an article teeming with errors, inaccuracies, and distorted reasoning, have restricted even more the alleged realm of cryptozoology by stating that "among mammals the cryptozoological search is largely for supposed primates" (see George Gaylord Simpson, 1984, *Mammals and Cryptozoology*, *Proceedings of the American Philosophical Society*, Vol. 128[1]: 1–19).

My checklist, based on a careful 35-year analysis and evaluation of more than 18,000 references from books, articles of periodicals, and personal communications, is, of course, neither restrictive nor fixed, but it gives a fair idea of the extent of cryptozoological interests. It contains between 82 and 95 unknown species or subspecies of animals. Amongst them, from 24 to 35 are aquatic forms (in seas and freshwater environments), and from 48 to 60 are terrestrial forms. The aquatic forms seem to be distributed as follows: 14 to 15 mammals (pinnipeds, archeocetes and sirenians), five to 15 reptiles or amphibians (crocodiles, snakes, mosasaurs, etc.), three to five fishes (eels, sharks, catfishes, etc.) and two mollusks (a squid and an octopus). The terrestrial forms seem to be distributed as follows: 40 to 51 mammals,

four to six reptiles (lizards and snakes), three birds, and one to three flying vertebrates *incertae sedis* (bats and/or pterosaurs?). Of the 40 to 51 terrestrial mammals, only 17 to 24 seem to be primates (men, apes, monkeys, and lemurs). The other ones—between 23 and 27—obviously belong to other orders, such as monotremes, marsupials, chiroptera, edentates, carnivora (bears, dogs and cats), proboscidiens, rodents or hyracoids, odd-toed ungulates (horses and rhinos), and even-toed ungulates (deer and antelopes). Terrestrial animals represent, thus, only between 58 and 63 percent—*less than two-thirds*—of the *total* number of forms cryptozoology is interested in, and primates represent only between 31 and 36 percent—*about a third*—of the *mammalian* forms cryptozoology is concerned with.

This clearly shows that the very premises of both Groves' and Simpson's criticisms are false, and, incidentally, that the generally received opinion that cryptozoologists are only tracking Bigfoot, the Yeti, and Nessie, is plain nonsense.

Despite his overt hostility towards cryptozoology, Groves actually brings grist to its mill by giving the impressive list of what he calls "large and terrestrial" mammals discovered over the past decennium only: no less than six. The same average would bring up to 27 the number of similar species described since the discovery of the kouprey in 1937, and this, indeed, is approximately what has been achieved. What a splendid harvest, and in the sole restricted field of mammalogy! No need for such a lament as, "Forty years since the kouprey is too long!" And I am surprised that, by extrapolating in the light of his own census, Groves thinks that future discoveries will mostly be "relatively unexciting." This is a valuation I would not have expected from such a brilliant zoologist.

Since Groves, as did Simpson, specializes in mammalogy, just as, incidentally, I did myself, let us, for a proper restatement of the question, limit ourselves to the mammals about which we are most informed. I would now like to elaborate even a little more upon the refinement Groves craves, as far as the rate of discovery of the larger ones is concerned, by taking a broader view of the situation and examining it throughout the present century. This will give me the opportunity to answer not only Groves' undue pessimism, but also some of Simpson's incorrect assertions (among others: "No large living mammals, not to mention dinosaurs, has been discovered in Africa since 1901"), and even a recent claim by the late Ralph Wetzel that his own live *Catagonus wagneri* was "the first medium-to-large-sized mammal to be added to the world list of living mammals since the okapi was described in 1901" (see Ralph M. Wetzel, 1981, *The Hidden Chacoan Peccary*, *Carnegie Magazine*, Vol. 55[3]: 1–36).

For the enlightenment of everybody, may I thus submit hereafter a list of what Wetzel calls "medium-to-large-sized" mammals discovered since the beginning of this century. It does not include animals weighing less than the

80 pounds ascribed to *Catagonus wagneri*, such as five out of the six listed by Groves. Nor does it even include new subspecies, such as the mountain gorilla (*Gorilla gorilla beringei* Matschie, 1903), the northern race of the white rhinoceros (*Ceratotherium simus cottoni* Lydekker, 1908), or the golden takin (*Budorcas taxicolor bedfordi* Thomas, 1910), in spite of the exemplarity of their spectacular discoveries. Nor does it include new forms which are still considered controversial, such as the pygmy elephant (*Loxodonta pumilio* Noack, 1906), the pygmy gorilla (*Gorilla* [*Pseudogorilla*] *elliotti* Frechkop, 1943) or the present-day Neanderthal Man (*Homo neanderthalensis pongoides* Heuvelmans, 1969). My list is strictly limited to full species appearing in *Mammal Species of the World*, edited by Honacki, Kinman and Koepl, 1982 (Lawrence, Kansas, Allen Press/Association of Systematics Collections), and which gives the references of all original descriptions.

Here are, first, the new bulky terrestrial species: *Okapia johnstoni* Sclater, 1901, the short-necked giraffid of the Congo forest; *Hylochoerus meinertzhageni* Thomas, 1904, the giant forest hog of Central Africa; *Tragelaphus buxtoni* Lydekker, 1910, the mountain nyala of Ethiopia; *Bubalus quarlesi* Ouwens, 1910, the mountain buffalo of the island of Sulawesi, in Indonesia; *Pan paniscus* Schwartz, 1929, the pygmy chimpanzee; *Bos sauveli* Urbain, 1937, the kouprey or grey-ox of Cambodia; *Pseudois schaeferi* Haltenorth, 1963, a wild goat of western China; and *Catagonus wagneri* Rusconi, 1930, the large fossil peccary found live in 1975. (Mark! Three "large living mammals" discovered in Africa since 1901.)

The list of new bulky aquatic species is naturally more impressive: *Arctocephalus galapagoensis* Heller, 1904, a sea-lion of the Galapagos Islands; *Monachus schauinslandi* Matschie, 1905, a seal of the Hawaiian Islands; *Mesoplodon bowdoini* Andrews, 1908, Bowdoin's beaked whale; *Phocoena dioptrica* Lamille, 1912, a perhaps circumpolar bicolored porpoise; *Mesoplodon mirus* True, 1913, True's beaked whale; *Lipotes vexillifer* Miller, 1918, the white dolphin of Tungting Lake, China; *Indopacetus pacificus* Longman, 1926, a beaked whale of Australian waters; *Tasmacetus shepherdii* Oliver, 1937, a beaked whale of New Zealand waters; *Lagenodelphis hosei* Frazer, 1956, a dolphin of Borneo waters; *Phocoena sinus* Norris and McFarland, 1958, a Baja California porpoise; *Mesoplodon ginkgodens* Nishiwaki and Kamiya, 1958, a beaked whale of Japanese waters; and *Mesoplodon carlhubbsi* Moore, 1963, a North-Pacific beaked whale.

On the whole, 20 new species of medium-to-large-sized mammals, of which seven belong to new genera (*Okapia*, *Hylochoerus*, *Lipotes*, *Indopacetus*, *Tasmacetus*, *Lagenodelphis* and *Catagonus*), have been discovered since the beginning of the 20th Century; 10 during its first quarter, four during the second, and six during the third. This indicates that, if the rate of discovery of such large animals is decreasing, it is doing so only very slowly.

To make a long story short, during the present century a new species of medium-to-large-sized mammal has been discovered on the average of every four years, and even a little less. This cannot be taken as a very exceptional event; it is rather routine. So one must be either ignorant, dishonest, or emotionally prejudiced against cryptozoology to state about the very same period, as Simpson did in the last, alas, article he wrote (see Simpson, 1984, above), one unworthy of his paleontological career: "There has been no definite and objective discovery of any living taxa that were previously unknown or hidden in the cryptozoological sense. The probability of future objective evidence of true zoological taxa is small but real for species and is decreasing to zero higher in the hierarchy of superspecific taxa."

BERNARD HEUVELMANS

ON DATA SELECTION AND ANALYSIS

(Comment on Heuvelmans, *Cryptozoology*, Vol. 2: 1-24)

While we do not necessarily disagree qualitatively with the conclusions of Heuvelmans' article, i.e., that many species remain to be discovered and that these probably will not be land vertebrates, his methods of arriving at this conclusion deserve some comment.

His major data base (Table 1) consists of 12 columns of numbers supposedly representing the best estimates of the numbers of described species in various animal groups at different times from 1758 to the present. However, the estimates for 1850, 1900, 1950, and 2000 are Heuvelmans' own, "based on the graphical expression of their data taken altogether, and occasionally corrected in the light of more recent or more accurate numberings of specific groups." Although we are willing to believe that these numbers represent "corrections" by the author with the intent of allowing for changes in taxonomic convention, he gives no documentation of their source or method of derivation, and leaves himself open to possible accusations of bias in the direction of his preconceived notions. Furthermore, the column for 1928 is clearly not comparable to the others since it includes subspecies as well as species (thus accounting for most of the "irregularities and apparent absurdities" seen in Figure 1).

Thus, there appear to be seven (not 12) estimates involving the same parameters from reliable and independent sources that might be used legitimately to predict how many species remain to be discovered in these particular taxa. What happens when accepted methods of analysis are applied to these data? We plotted the number of registered species against time for the 18 groups chosen by Heuvelmans and attempted to fit them to linear,

exponential, and polynomial models. The results are quite striking. 1) All showed a statistically significant ($p < .05$) increasing linear relationship, and 10 were highly significant ($p < .01$). 2) All showed a statistically significant ($p < .05$) increasing exponential relationship (for 17 of them, p was $< .01$). 3) All showed a significant fit to a polynomial function. Of these, 16 were increasing with a probability of .01 or less, and one (Protochordates) showed a decreasing trend with the same probability. Birds showed a decreasing trend with $p < .05$. 4) Over all comparisons, an increasing polynomial model provided the best fit to the data in 11 cases, a decreasing polynomial model in one (the Protochordates), and an increasing exponential model provided the best fit to the data in six cases. Thus, in direct contrast to Heuvelmans' conclusions, our analysis of what we judge to be the admissible data shows that the future for cryptozoology is rosy indeed in all taxonomic groups save one!

One could comment that applying reasonably sophisticated analytical techniques to these numbers is patently absurd, and we would agree. Common sense dictates that the information at hand is simply not up to the task of predicting the number of animal species yet to be discovered (the last legitimate estimate was Mayr's, made nearly 40 years ago). However, our purpose in doing this exercise was not to make such an estimate, but to show that the conclusions in this paper do not really follow from the data base presented.

Other criticisms can be made as well. One of the more serious is that most of the "upsetting discoveries" cited by Heuvelmans are not accompanied by a citation to the original literature, making checking his interpretations rather difficult. There are also numerous errors of fact. For example, the Devonian ended about 440 (not 400) million years ago, according to the most recent estimates (see W. B. Harland et al., 1982, *A Geologic Time Scale*, Cambridge University Press, Cambridge); and the giant panda is a bear, not a procyonid (see James H. Honacki et al., eds., 1982, *Mammal Species of the World*, Allen Press/Association of Systematics Collections, Lawrence, Kansas).

In conclusion, the scientific soundness of this paper can be seriously questioned. Cryptozoology should adhere to rigorous standards of data selection and analysis.

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FASHION AND NOVELTY—NOT TO BE CONFUSED WITH TRUTH

(Response to Brussard and Wright)

As the computer craze is reaching its peak, it is to be expected that enthusiasts would try to solve all zoological problems by purely mathematical methods. This, however, would be forgetting a little too soon the bitter disappointment inflicted upon zoologists by the use of numerical taxonomy, however promising it looked. Brussard and Wright's attempt at predicting the number of animal species yet to be discovered belongs to the same wave of scientific fashion. However, to support the alleged superiority of their own method, they make charges against me which are absolutely unfounded, and will appear imaginary to anyone who reads my paper carefully. Let us examine them seriatim.

Concerning the "corrections" I brought to the published estimates of the number of described species in various animal groups, Brussard and Wright accuse me of giving "no documentation of their source or method of derivation." But *all* my numerous sources of information were, in fact, mentioned in my paper (p. 13), and my method of derivation has clearly been defined as a graphic treatment of all data taken altogether; that is, the classical creation of "curves obtained by a judicious smoothing of the original zigzag plots" (p. 17). I even emphasized that, as they point out accusingly, "the column for 1928 is clearly not comparable to the others," and I gave my reasons for using it anyway (p. 15).

It is quite possible that, through their own method of mathematical analysis of the various data—which I selected as rigorously as possible, since they were the only available ones—Brussard and Wright arrived at more accurate, more "striking" and even slightly different results from mine—which were obtained by an admittedly old-fashioned method, but still rewarding when the "essential aim has been to obtain orders of magnitude and to make comparisons" (p. 22). However, we unfortunately have to take their word for this, as they just deliver the rough results of their analysis without giving any corresponding figures, and they do not even give us much detail about their *modus operandi*. The scientific soundness of such a procedure can certainly be more than questioned.

Brussard and Wright boast that their "analysis of what [they] judge to be the admissible data shows that the future for cryptozoology is rosy indeed in all taxonomic groups save one [the Protochordates]!" Is this not almost exactly what I concluded myself when I said that "one may entertain high hopes for future discoveries, providing full justification for cryptozoological research" (p. 22), except probably in the realm of birds, not to mention the here irrelevant Protochordates? This, nevertheless, does not prevent them from disingenuously stating that what they have discovered is "in direct contrast to Heuvelmans' conclusions."

As a matter of fact, Brussard and Wright sealed their own fates when they wrote—quite judiciously for once—"One could comment that applying reasonably sophisticated analytical techniques to these numbers is patently absurd, and we would agree." So do I.

One of their more serious "criticisms" is that most of the "upsetting discoveries" listed by me were "not accompanied by a citation to the original literature," thus making the checking of my interpretations "rather difficult." As a matter of fact, taking for granted that I was addressing well-informed zoologists who know perfectly well where to find the original reference to any zoological discovery without the slightest difficulty—except the very recent ones, of course—and reluctant to double the amount of already numerous cited references, I limited myself to general works giving numerical estimates for whole groups of animals. (The Editor, perhaps incautiously, added some references on particular discoveries—but, alas, not all. This explains a disharmony which is unfortunate and certainly open to legitimate criticism, but for which I am not to blame.)

Furthermore, when Brussard and Wright claim that "there are also numerous errors of fact" in my paper, they are themselves making gross errors. They only mention two of these "numerous errors," but they certainly made bad choices.

They blame me for having called the giant panda "the largest of the Procyonidae," which is my personal opinion, an opinion I share with other prominent mammalogists. Brussard and Wright rashly state that this animal is a bear because it is so classified in the recent mammal reference work by Honacki et al. If they had attentively read this excellent book (especially pp. 250 and 252), they would have noticed that the nature of the giant panda is still a very controversial question. Somebody is not necessarily right simply because he has been the last to speak.

Brussard and Wright also blame me for having written that the Devonian ended about 400 million years ago, when, according to "the most recent estimates" (fashion once again!), it ended about 440 million years ago. I cannot find where I expressed this in my paper, but I could very well have given such an approximate estimate, to which, incidentally, some reputable paleontologists still adhere. I happen to remember that, when I started studying zoology at the Brussels Free University some 50 years ago, the end of the Devonian was estimated at 360 million years ago by the best authorities. This estimate had already increased by 30 million years when I obtained my Ph.D. in 1939, hardly 5 years later, and it has regularly changed since, going sometimes up and sometimes down. In 1949, for instance, it was given as 275 million years by such authorities as Oakley and Muir-Wood (see *The Succession of Life Through Geological Time*, London, British Museum [Natural History]). And there is, of course, no reason why it should not be often "corrected" again in the future. So it is much more cautious and realistic to

stick deliberately to a rather vague estimate than to believe that the last one is the definite and correct one.

Brussard and Wright's overprecision in this matter reminds me of the natural history museum attendant who used to tell visitors that a particular dinosaur's skeleton was 175,000,037 years old. When someone asked him by which technical methods such accuracy could be attained, he promptly answered: "Well, when I got this job I was told that this reptile was 175 million years old, and I've been working here now for 37 years."

BERNARD HEUVELMANS

GOLDEN MONKEY VERSUS WILDMAN: A QUESTIONABLE COMPARISON

(Comment on Poirier et al., *Cryptozoology*, Vol. 2: 25–39)

There are two issues I am critical of in this article. The first is the coauthors proposing the rare golden monkey, *Rhinopithecus roxellanae*, as an explanation for Wildman sightings without presenting explicit anatomical details of the monkey. The questionable data, that is, eyewitness reports of Wildman, include specifics, such as: height (from 1.2 to 2.5 meters), purported footprints (up to 48 centimeters), and that it is bipedal without a tail. No estimate of body weight was given.

In comparison, we are told the golden monkey is a rare animal that few people have seen, and it therefore could be mistaken for a Wildman. Two isolated incidents are presented, relating how a golden monkey was thought to be a Wildman by the local people. The two examples, though, are offset by the coauthors' earlier statement that the morphological traits of the Wildman are based on some 300 sightings. It is inconceivable that such a large number of people are possibly mistaking monkeys for Wildman, and, therefore, the two incidents do not support the mistaken identity hypothesis.

Oddly, the body dimensions of the golden monkey are never given, which immediately weakens the comparison, if not invalidates it. Yet, evidently, it was important that the coauthors included a photograph of the monkey. However, no prototype composite sketch of the supposed Wildman is furnished to aid in the comparison.

Also, the article does not state whether *Rhinopithecus roxellanae* is bipedal, semi-bipedal, or quadrupedal. Foot size is omitted as well. The article leaves many unanswered questions, such as: Does the monkey have a tail? Is it primarily arboreal or is it a cliff-dweller like the Ethiopian gelada (*Theropithecus gelada*)? Do they travel in troops, or are they mostly solitary animals? None of these issues is properly addressed and quantified, yet they

are vital when attempting to establish anatomical comparisons involving two separate species.

Grzimek's *Animal Life Encyclopedia* (1972) classifies *Rhinopithecus roxellanae* as a snub-nosed monkey that forms large groups, lives in cold, high mountain regions, and has thick, gleaming golden fur. It also states that this cercopithecoid has a massive skull, a robust figure, and rather long arms. The head-rump length is 52 to 83 centimeters (less than two and a half feet in length for the largest male of this species). It has a tail length of 9 to 14 centimeters. In addition, the golden monkey is quadrupedal; no foot-size was given.

None of the above dimensions of *Rhinopithecus roxellanae*, in a practical or scientific sense, can be comparable to a two-to-three-meter tall, tailless biped that leaves 30 to 48 centimeter footprints. Apparently, two types of Wildman footprints suggest two separate species, a larger one as mentioned above, and a smaller type with 20 centimeter feet, which resemble those of an ape or monkey rather than a man. Still, most likely, the smaller prints are far too large for a golden monkey's feet in proportion to its overall body size. Therefore, because of insufficient data on the golden monkey and the two monkey anecdotes offered as supporting evidence against the existence of Wildman (when 300 Wildman reports apparently did not confuse eyewitnesses), I find the comparison invalid and confusing.

It is also interesting to note that Poirier uses himself as a comparison to a Wildman. He relates a personal incident where rural Chinese children in a remote district mistakenly viewed the Western stranger as a Wildman, thus implying the possibility of other mistaken identities among the 300 reports on record. But to compare the cognition of children, whose range of fantasies remains limitless in every culture, to detailed reports by responsible adults, is another gross generalization that has no bearing in scientific reporting. I contend, that, like the monkey comparison, it is a poor basis for supporting the argument that non-Chinese people and rare monkeys are being mistaken for Wildman.

The second issue in the article is that of a contradiction by Poirier and his coauthors. They state that the fossil ape *Gigantopithecus* "perhaps stood 6 feet tall" (p. 38). However, an incongruity exists between the above quotation and Poirier's own book entitled *Fossil Evidence: The Human Evolutionary Journey*, which was used as a text when I was enrolled in a physical anthropology course at Williams College some years ago. It states: "*Gigantopithecus* is judged to be an animal weighing over 600 pounds and standing nine feet tall" (see Frank E. Poirier, 1973, *Fossil Evidence: The Human Evolutionary Journal*, C. V. Mosby, St. Louis). Interestingly, Pei Wen-Chung, the Chinese scholar and authority on *Gigantopithecus*, believed the giant ape to be about 12 feet tall. Understandably, there is always academic con-

trovery and discrepancies when numerous skeletal parts are missing, as in *Gigantopithecus*.

Because of the interdisciplinary sciences involved in cryptozoological research, it is important to be consistent with the facts, realizing that updated research can, at times, change what was once considered a norm. Yet, in speaking most respectfully of Poirier and colleagues, I still feel that, in making comparative statements, they are incomplete, and thus misleading. In addition, their supportive material at times appears somewhat tailored to fit their hypothesis, however unintentional this may have been.

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A GOLDEN CLARIFICATION

(Response to Lapseritis)

Lapseritis correctly points out a number of potential problems in our Wildman article. I have no particular quarrels with what he has to say. However, I would like to clarify our position.

1. While we noted that there are some 300 sightings of the so-called Wildman, I wish to note again that we have little faith in many of these. The reports are highly disparate, and it is not at all clear that all reports are of the same phenomenon. It is our opinion that many sightings of "strange" creatures have been generalized into the category of "Wildman." While many, perhaps most, reports of the Wildman are not mistaken identities of the snub-nosed golden monkey, it was our intention to note how *some* of the supposed 300 reports have been misconstrued.

2. I do not think we implied that the golden monkey has the same physical dimensions as the reported Wildman; they do not. Snub-nosed golden monkeys are much smaller both in height and weight, they have very long and obvious tails, their hands and feet in no way approach the dimensions reported for the Wildman, and they are largely arboreal. The snub-nosed golden monkey moves terrestrially, but often stands bipedally. Under normal conditions, one could not mistake a golden monkey for the so-called Wildman. However, how many sightings of the Wildman were made under normal conditions? The other point we made was that the ranges of the so-called Wildman and the snub-nosed golden monkey have considerable overlap.

3. The only morphological resemblance we made between the Wildman and the snub-nosed golden monkey concerned the identification of hair. The hair that I obtained, that supposedly came from Wildman, unmistakably belonged to a golden monkey. Continuing news articles today refer to Wildman hair samples. The hairs being cited, however, are almost certainly the same ones from which our sample derived. That hair definitely belongs to the snub-nosed golden monkey. There are no hair samples of which we are aware which warrant changing our view.

4. Concerning the relegation of myself to the Wildman status, I should note that adults as well as children referred to me as "the Wildman." Our intent was to show how something, or someone, who looks different, or is in some way unusual, can generate reports of strange creatures. We did not mean to imply that the cognition of children and of responsible adults is similar. Furthermore, we did not say, or imply, that all non-Chinese (in this case myself) and *some* snub-nosed golden monkeys *have* been identified as the Wildman.

5. The status of *Gigantopithecus* is still being debated. There are different height and weight estimates. I did, indeed, once report a weight of over 600 pounds and a height of nine feet. Based on newer estimates, including an estimate by some of my own students, such claims seem to be clearly inflated.

Despite my rejoinder, we welcome Lapseritis' comments, for they have provided an opportunity for clarification of our opinion. We hold firm in our conviction that the case for Wildman's existence has been poorly made, poorly documented, and is yet very speculative. We do, however, continue to have open minds, even if we have little hope for the existence of such a creature.

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REEVALUATION IN DOUBT

(Comment on Heaney, *Cryptozoology*, Vol. 2: 40-52)

Although Heaney and I correspond and exchange information on hominology, his article came as a complete surprise to me because he had never mentioned his investigation in this direction. I read his paper with mixed feelings: great admiration for the tremendous amount of work he put into

it, but also a certain sadness, even frustration, over the indefinite nature of his main conclusion.

The site of Heaney's "fieldwork" lies in the archives. Is this kind of research relevant and useful in cryptozoology? Very much so. Recently, archivist Graham Joyner produced a wealth of historical data supporting the possible existence of hairy bipeds in Australia. Heaney appears to have set himself a task in reverse: he took a country, in which the existence of relict hominoids has long been recognized by a number of researchers, and he examined the archives for a mention of one well-known sighting in that habitat. He found no mention of it; an absolute zero. The question is: what are we to make of it?

Heaney concludes that this puts Baradiin's sighting in grave doubt. For my part, I dare to doubt this conclusion.

Let us suppose Heaney had discovered Baradiin's testimony in the scholar's own handwriting. Would it have made the account any more credible? If we are to doubt the man's oral testimony, why should we trust him in writing?

As a matter of principle, the existence of a record in an archive is fine, but its absence gives us no clue. Yes, it *is* strange that Baradiin should have failed to mention the sighting in his diary and the letter to Zhamtsarano. But hominology is rife with cases that defy conventional logic. For example, a hunter in Oregon was once reportedly abducted and carried some distance by a Sasquatch, while another hunter watched the incident. The two men never discussed the event, let alone commit it to paper, for more than twenty years. Albert Ostman's famous story was written down 33 years after his purported kidnapping by a Sasquatch. The bulk of eyewitness accounts in our country, including the well-known ones by General Topilsky and Dr. Karapetian, were recorded from oral testimony long after the actual incidents. Another Russian doctor claims to have seen a whole Almas family in Mongolia in 1967, but never bothered to write it down.

So it *is* conceivable that Baradiin may have never committed his Almas sighting to paper. This conclusion seems more plausible to me than the supposition that the man was a shameless liar.

Even so, as Porshnev pointed out, "We do not rely on the testimony of this or that person. Any account can be in error or untrue. What matters here is that we have many accounts In the terms of the theory of probability, a sufficiently large number of independent reports, any one of which can be called into question, in summation produce a reliable result" (see *The Struggle for Troglodytes*, my translation).

Heaney remarks that "Porshnev may have been unwise to relate 'evidence' at third and even fourth hand . . ." Naturally, Porshnev preferred first-hand accounts when he could get them, and in Baradiin's case, as Heaney

knows, he couldn't. So he used Rinchen's second-hand and Rozenfeld's third-hand accounts, which were independent of one another.

Heaney amply demonstrates, and this is a salient feature of his article, that hominoid research has been conducted, not in a vacuum, but in the teeth of mankind's tragic history. But that, as the English say, is a different kettle of fish.

To sum up, Heaney's article is a marvel of meticulous work, and yet it seems to lead to a blind alley in probing the reality of Baradiin's sighting.

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CORROBORATION LACKING

(Response to Bayanov)

Recording the events that took place during his expedition was an essential part of Baradiin's work, in contrast to those supplying the other anecdotes mentioned by Bayanov. I do not accuse Baradiin of lying. I merely point out that what others have said and written is not corroborated by his own account. It is as important and revealing to study the manner in which information is transmitted (and altered in transmission) as it is to ponder on the data themselves, especially in a field such as cryptozoology, where one person's fiction or supposition so often becomes another's fact.

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LIVING ANIMAL OR FAKED RIDGES?

(Comment on Krantz, *Cryptozoology*, Vol. 2: 53-81)

Grover Krantz' findings concerning the alleged Sasquatch footprints with dermal ridges seem to lead to a clash of dogmata. It is a strongly held professional opinion among forensic experts that fingerprints cannot be successfully manufactured or altered. Indeed, fingerprint identification has be-

come one of the cornerstones of the criminal justice system for this very reason.

However, the Walla Walla prints put this idea at loggerheads with the equally cherished belief held by most zoologists, anthropologists, and folklorists that Sasquatch does not exist. At least one of these tenets must be false.

Once forensic scientists become aware of the vested interest they now have in the existence of the Sasquatch (and the Society can perhaps hasten that awareness), it seems to me that they cannot leave the question unresolved, for it is manifest that either a living animal made the Walla Walla prints, or someone has successfully faked dermal ridge patterns, with all that that implies for law enforcement and national security.

Establishing the existence of Sasquatch would be the least traumatic resolution of this issue from the viewpoint of forensic science, and it would also at least raise the possibility of funding for Sasquatch research.

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WHOSE DERMAL RIDGES?

(Comment on Krantz, *Cryptozoology*, Vol. 2: 53–81)

Krantz presents evidence of dermal ridges on a set of Sasquatch footprints, and emphasizes their importance in confirming the nature of the Sasquatch as an unknown primate. Krantz' analysis is of great significance, and opens up a new dimension in the study of Sasquatch prints. This development has been widely publicized, by Krantz himself at meetings, in the publications of the International Society of Cryptozoology, and in the popular press (see Scott Forslund, 1983, *The Nature of the Beast*, *Pacific Northwest*, March; see also Patrick Huyghe, 1984, *The Search for Bigfoot*, *Science Digest*, September).

In view of the importance given to dermal ridges as an element of proof of the existence of large, wild primates in western North America, it is essential that each link in the chain of evidence which ties the ridges to the Sasquatch be solidly forged. Krantz has shown that the ridges seen on the footprints found near Walla Walla, Washington, on June 16, 1982, are indeed those which should be expected on the foot of a primate; he quotes reports from a series of experts on dermatoglyphics who support this view. Granting

that part of the argument, it still remains to be shown that the footprints were actually made by a Sasquatch. This point was not addressed adequately by Krantz, and it is my contention that there are a number of elements in the tracks themselves, and in the circumstances which surround their discovery, which throw severe doubt upon their authenticity. Some of these points have already been alluded to by Forslund. In view of its importance, the issue deserves airing in this professional forum.

The circumstances surrounding the discovery of the dermal ridge-bearing tracks have been described in *The ISC Newsletter* (Vol. 1 [2 & 3]) as well as by Forslund. To summarize briefly, the tracks were discovered on June 16, 1982, by Paul Freeman, a temporary employee hired by the U.S. Forest Service to patrol the Walla Walla watershed area during that summer. Freeman was accompanied at the time by Bill Epoch, another watershed rider. Following Freeman's report of his discovery, a team which included Rodney Johnson, a Forest Service wildlife biologist, and Joel Hardin, a tracker for the U.S. Border Patrol, was dispatched to the site by the U.S. Forest Service to examine the prints. I have consulted copies of the reports filed independently by Johnson and Hardin shortly after viewing the tracks; I also interviewed Mr. Hardin at length at his home in Bellingham in the spring of 1983. What follows is based on a synthesis of these various sources.

The first of three sets of prints was discovered by Epoch, following Freeman's suggestion that they eat their lunch near a bog (Elk Wallow), next to the Low Creek Trail. It included approximately 18 prints over a distance of about 40 ft, beginning in the soft mud adjacent to the bog, extending over a small rise, down to the other side, and thence into dense herbaceous vegetation. According to Krantz (p. 54) the tracks were made by two individuals; one of the sets of tracks matched those observed in connection with a sighting made by Freeman the previous week a few miles away. Both Johnson and Hardin reported that the tracks made in the mud were *shallow*: observers in boots sank deeper in the mud than the reputed Sasquatch tracks, and a pencil was easily poked in the tracks down its whole length without touching solid bottom. Those tracks seen over the small rise were on a dry trail, on which were strewn small stones and fir needles, usual forest litter. It was observed that these prints were free of pebbles and needles, as if they had been picked out or rubbed away by buffing the print in.

After this discovery, Freeman rode down to the caretaker's cabin to give notice of the find. On his way back, less than an hour later, he discovered a group of 8 prints, overlying his horse's tracks, as if the creature had been following him down the trail. On the next day, these tracks were still clearly visible, clustered around a small seep on the track. Two or three tracks showed up on each side, approaching and departing from the seep; a couple of prints were made in the soft mud of the seep itself. Other tracks (of people, bears, and horses) could be seen nearby and far from the seep on both sides.

The Sasquatch traces, however, appeared out of nowhere and disappeared just as abruptly. Dermal ridges were clearly visible in those prints made in the soft mud, and the casts made from those prints are the ones discussed by Krantz. Hardin's comment on the toeprints was that, whoever made them, "took very good care of their feet." There are always small cracks and callouses in the feet of people who walk barefoot all the time; none were visible in those prints. Furthermore, the prints laid in the soft mud were relatively shallow; people in boots made deeper impressions.

The third set of tracks was not examined by the U.S. Forest Service party, and is not discussed here.

Hardin and Johnson concluded that the prints could not have been made by a heavy, wild creature because of the following points:

- 1) When in soft mud, the prints were too shallow;
- 2) The tracks were in a straight line, and did not show a natural swing on an irregular trail;
- 3) The stride was unnaturally uniform (4 ft) on both uphill (2–3% slope near the seep) and downhill stretches (8–25% in the dry area near the bog);
- 4) The tracks showed exactly the same weight and body angle pressure on the heel and ball of the feet on uphill and downhill segments;
- 5) There was no evidence of slipping of the heel on sloping areas;
- 6) The dermal ridges were abnormally pronounced, and the rest of the foot surprisingly free of blemishes for an habitual barefoot walker;
- 7) The tracks appeared and disappeared abruptly at both locations, with no sign showing approach or departure from the area, in contrast to other human and animal tracks in the same vicinity;
- 8) Tracks on the dry trail near Elk Wallow were free of fine forest litter, as if it had been brushed aside when making the tracks.

We note that neither Hardin nor Johnson dismissed the possibility that the tracks might belong to a Sasquatch merely because of the presence of dermal ridges, as Krantz intimates (p. 59); the latter were found surprisingly fine for a barefoot creature, but the authenticity of the prints was rejected mainly on the basis of the other points mentioned above.

Hardin's opinion is that "the tracks were a very clever attempt to perpetrate a hoax," and that whoever made them, using some type of soft plaster or rubber-coated foot, had to be a person with normal access to the area, and with considerable interest in Bigfoot. Johnson observed that "someone went to a great deal of trouble and detail. The print-marks on the foot would have to be very hard to duplicate."

We may also wonder at Freeman's exceptional success at finding Sasquatch footprints, not just once, but repeatedly, in an area where none had ever been observed before. Or again, in view of the stench often reported by Sasquatch witnesses, such as by Freeman himself the previous week (see Forslund, 1983, above), at the absence of any smell and of any reaction on

the part of his horse when Freeman discovered the fresh set of prints around the seep. Further, the identification of some of the impossibly shallow prints in Elk Wallow with those made by the Sasquatch seen a week before by Freeman raises doubts as to the veracity of that sighting.

What is to be concluded? Was it a hoax? Did someone other than a Sasquatch make the prints? The tracker and the biologist who saw the tracks certainly thought so. Their reports cast severe doubt on the authenticity of the tracks. Although they both agree with Krantz that fabrication of the prints would have been difficult, this does not mean that a hoax would have been impossible; only difficult.

One must agree with Krantz that "the implications of the tracks, if authentic, are profound." However, it is not the implications which are in doubt, but the authenticity. Although Krantz claims not to be "concerned with the truth or accuracy of Freeman's claimed sighting" or of the tracks themselves, his analysis is relevant only if the tracks are indeed authentic. It would thus seem premature at this stage to claim that the dermal ridges seen on the Walla Walla prints add a conclusive element to Sasquatch lore.

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ET TU, RENE?

(Response to Dahinden)

Dahinden raises an aspect of the Walla Walla tracks that deserves an airing. My paper dealt only with the anatomy of the footprints, on the assumption that the circumstances of their impression and discovery were clear enough for all practical purposes. This may not be the case, as it evidently has bothered some people. It did come as a surprise to me, however, that a Sasquatch supporter was a major objector.

We might first note two quotations from Dahinden's comments:

... it still remains to be shown that the footprints were actually made by a Sasquatch (second paragraph).

... his analysis is relevant only if the tracks are indeed authentic (last paragraph).

This attitude misses the whole point of my analysis of these tracks. I did not attempt to prove that they were authentic in terms of discovery, location, impression, stride, etc., for the simple reason that this is *not possible*. No amount of "proof" of this kind will satisfy the debunker as to the authenticity

of *any* Sasquatch tracks. This has been attempted by many investigators in the past, but to no avail. The scientific community either points out some inadequacy in the evidence, or simply ignores it entirely. Yet, it is just this scientific community that must be convinced if the Sasquatch is to become known, studied, and protected.

Granting the futility of that approach, it seemed that, in this case, the evidence of the tracks themselves might offer a quite different form of "proof." I was fully aware of the questions regarding the circumstantial authenticity of these tracks, as well as the counter arguments. It seemed pointless to argue the pros and cons—this has been done many times before, and nothing has ever come of it.

Indeed, if one could demonstrate that any poor quality set of tracks was authentic, then the Sasquatch would be proven to exist. Dermal ridges are hardly needed if there is *direct proof* that it was a Sasquatch that made the tracks.

If anyone has such evidence, they have obviously failed to convince the scientists concerned. We must conclude that either: 1) such evidence does not exist; 2) the evidence has not been presented; or 3) the scientists have not looked at it. It makes little difference which of these are true—the fact remains that the scientific establishment has not recognized any evidence of Sasquatch tracks.

Dahinden recounts a number of scientific objections from Hardin and Johnson that challenge the authenticity of the tracks at Elk Wallow. Some of these are easily answered; others are not. We must remember that the only alternative to the Sasquatch as the source of these tracks is a human hoaxter. Dahinden makes no claim to have interviewed Freeman, whom he obviously implies is involved in the presumed hoax.

Hardin and Johnson's first point, about the shallowness of the tracks, is oddly contrasted with the well-known depth of imprints seen on the road the previous week. The same foot design is easily recognizable from both sites. Also, one track from Elk Wallow is actually 10 cm deep, while another is centered on a large stone. I wonder if a pencil probe is a good test of the ground's resistance against large foot impressions. That the men's boots sank deeper argues strongly for hoaxtering, but I have seen no photographs (among the many taken) that clearly illustrate this claim. If such photos are available, they should be published.

Points two through five relate to the evenness of stride and imprinting in the face of differences in the terrain. A human, using fake feet, would also be expected to show variations under the same circumstances—in fact, even more so. What is used to argue against the Sasquatch in these four points actually serves even more strongly to rule out a human hoaxter. The only kind of animal that might be expected to stride in such a regular manner

would be one so large that its strength and inertia would make it relatively unaffected by variations in the terrain.

The claim that the feet were free of blemishes (point six) is incorrect, and is easily checked. Several dermatoglyphics experts who studied the tracks under laboratory conditions for many days, have commented on the skin damage, irregularities, and scars they observed. This originally did not seem pertinent to my analysis. The dermal ridges were, in fact, developed and worn, according to these experts, in a manner consistent with habitual bare-foot walking.

The abrupt appearance and disappearance of the footprints (point seven) puzzles me too. If other tracks were so easily observed at that location, as claimed, why did they not also find the shoe prints of the presumed hoaxter that must have correspondingly disappeared and reappeared?

Point eight concerns tracks on the dry trail that lacked forest litter. Again, it would help if appropriate photographs were available to illustrate this claim. It may be that any small objects were crushed and/or pressed into the ground under the weight of these feet. Quite possibly, there is a real problem here.

Perhaps the most telling aspect to the above criticisms was relayed to me by three U.S. Forest Service employees (who must remain nameless). According to them, Joel Hardin was heard joking about the "fake" Sasquatch tracks *before* he went to the site to examine them. It seems a bit incongruous that Dahinden, who is known to accept the reality of Sasquatch, bases his position so much on the views of a man who thinks it is not real. I have no information on Johnson's opinion on this subject prior to their study.

Dahinden wonders about Freeman's success in finding so many tracks "... in an area where none had ever been observed before." John Green lists three reports of sightings, and two of tracks in that area (see John Green, 1978, *Sasquatch: The Apes Among Us*, Hancock House, Saanichton, British Columbia). In addition, I have heard of two other previous sightings near the area.

In less than a year after this event, Freeman reported finding many more tracks, and he cast those of two, possibly three, individuals in addition to the original two. During that same time, three other parties of hunters/campers that I know of have also found tracks there. One of these, unknown to Freeman, claims to have seen one of the creatures, as well as its tracks, on two occasions. His description agrees closely with Freeman's, including details that were never published. No U.S. Forest Service personnel have reported further tracks. This is not surprising; the unpleasantness generated by Freeman's experience made it quite clear that the authorities did not favor any more such discoveries.

Dahinden also wonders why there was no smell or reaction by Freeman's

horse at the fresh Elk Wallow tracks. John Green's records (see Green, 1978, above) show that fully 94% of all sighters reported no unusual odor. Freeman noticed no smell at Elk Wallow, but says his horse *did* react, almost throwing him. It should be noted that this was not the same individual Freeman said he saw and smelled the previous week.

As to the *difficulty* of fabricating these footprints, all parties agree in principle. My own initial impression was that they could not have been faked, but I did not know enough to be sure. The claim that this would have been *impossible* stems mainly from the lengthy analyses by several of the best fingerprint experts in the country, and some from abroad.

We are still awaiting a plausible description of how this hoax might have been accomplished. From where were the skin imprints obtained that match no living species? What kind of fake foot was used that varied the depths of its metatarsal imprints and conformed to a 2 cm indentation from a stone it stepped on? How were the footprints so deeply impressed in at least some locations?

Early in our investigation of this event, many of us thought that these tracks might prove to be such definitive evidence that the Sasquatch would become generally accepted by science. This was not to be the case. If they had been accepted, then qualified primatologists would have taken over the whole area of Sasquatch research. All those people without proper primatological credentials (including myself), would have been relegated to the sidelines. All of the efforts by all of the workers in the Sasquatch field would have become no more than anecdotes.

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ON SASQUATCH FOOTPRINTS

(Comment on Krantz, *Cryptozoology*, Vol. 2: 53–81)

From experience, I long ago learned not to draw conclusions from photographs, particularly of casts, so I will say nothing about the photographs in Krantz' paper. All that I can say is that the height of 2.5 m, calculated from the stride, is more than enough to make me doubt the existence of such a manlike creature, for there is no authentic case of any manlike primate—not even *Gigantopithecus*—ever having reached such a height; and if it did its feet would have to be a good deal longer than 37 or 38 cm.

Finally, a height of 2.5 m would put a rapid end to such a manlike form, for it would mechanically and functionally fall apart from the stresses and strains acting upon it. This was dramatically evidenced in a young American male who, some forty years ago, was much publicized in the media for his excessive height, and who died shortly afterwards. Unfortunately, I cannot recall the details, but anyone interested in the subject should have no difficulty tracking the case down.

Meanwhile, I would recommend the reading of the article on "Giants" in George M. Gould and Walter L. Pyle, *Anomalies and Curiosities of Medicine*, Philadelphia, W. B. Saunders, 1896, and J. B. S. Haldane's "On Being the Right Size," in his book, *Possible Worlds*, London, Chatto & Windus, 1927. Some years ago, *Scientific American* published an article on the biomechanics of size, but, again, I cannot recall the exact reference.

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ON GIANT MEN

(Response to Montagu)

The original footprints were destroyed by casting, and would soon have lost their detail anyway. We are fortunate that good quality casts were made at all, and doubly fortunate that these reached scientific hands. My resources have been strained by showing these casts to experts in the U.S., Canada, England, the Netherlands, and China. I then published a description and analysis, including expensive photographs rather than drawings. It is frustrating indeed to have all this dermatoglyphic evidence dismissed out of hand.

It may be useful now, instead, to comment on the point where Montagu and I agree—that the Sasquatch is not human. It is true that a 2.5 m tall human body "... would mechanically and functionally fall apart from the stresses and strains acting upon it." Unusually large people have mostly the same levers and other tissue ratios as in smaller people. With increasing size, simple geometry illustrates how surface (strength) increases faster than volume (weight), thus altering all strength to weight ratios.

When people report seeing an upright, bipedal, hominid-looking primate of this size, the *first* thing we can rule out is a massive, eight-foot tall man in a fur suit. Such a man could not easily stride by, leave well-spaced foot-

prints, and disappear into the forest. If the creature is real, then its size argues for considerable differences from the usual human anatomy.

In the absence of a body, we can only speculate on what most of these differences might be. But given many clear footprints, we have been able, at least, to study the design of the foot. As my previous publications have shown, this agrees closely with logical expectations for a hominid, but non-human, giant.

GROVER S. KRANTZ

THE CONGOLESE MOKELE-MBEMBE REPORT: ITS SCIENTIFIC CONTENT

(Comment on Agnagna, *Cryptozoology*, Vol. 2: 103–12)

The high peak of Agnagna's Field Report of the First Congolese Mokele-Mbembe expedition is the observation of the animal itself in Lake Telle, from a distance of about 240 meters. After stating that "the author was filming a troop of monkeys," Agnagna continues: "We were then able to observe a strange animal, with a wide back, a long neck, and a small head (Fig. 4). The emotion and alarm at this sudden, unexpected event disrupted the author's attempt to film the animal with a Minolta XL-42 movie camera. The film had been almost totally exposed already, and the author unfortunately began filming with the lens cap on."

The event described in this last statement is simply not possible. One cannot film with this kind of movie camera without seeing directly through the lens, and noticing that there is a cap or something obstructing the lens. The reason given by Agnagna as to why he could not film what he saw is evidently untenable. Thus, this published report contains a serious mistake, which needs to be clarified. This particular wrong detail could cast doubt and unreliability on the entire report, and, by implication, could make the journal itself questionable.

A problem with cryptozoology is that the literature is full of stories concerning the impossibility of producing some kind of proof, such as photos, because of an accident, but the situation is even more serious when the very details of the accident make the whole thing impossible. One of the aims of the journal *Cryptozoology* is to promote studies that are usually labelled as marginal, but are nevertheless serious. We have to make increased efforts towards scientific rigor, particularly in this field.

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Response by the Editor

The Editor apologizes for the error contained in Agnagna's Field Report. Agnagna, the author, was not responsible for the error. The text was originally in French, and the error resulted from a translation ambiguity and incorrect verbal information received from a third party in the Congo. Unfortunately, Agnagna was in the process of relocating to France for a year during the journal's editorial and production process, and the Editor was unable to communicate with him. By the time the error became known, the journal was already printed.

As a member of the journal's Editorial Board, Dr. Tassy has a legitimate concern, but he may rest assured that the Editor takes full responsibility for the error, which in no way involves Agnagna, or casts any doubt on his Field Report. A clarification of the error (the camera was accidentally set on "macro"—no lens cap was involved) was made in an interview the Editor conducted with Agnagna in Paris, in June of 1984, and which was published in the Summer, 1984, issue of The ISC Newsletter, Vol. 3[2].

The Editor.

MOKELE-MBEMBE OR TURTLE?

(Comments on Agnagna, *Cryptozoology*, Vol. 2: 103–12)

I am very open to the possibility that sauropods may be living in the Congo. However, the evidence presented to date, including the above report by Agnagna, still leaves me undecided about the existence or true nature of Mokele-Mbembe. As in all branches of science, alternate explanations for evidence (even "eyewitness" reports) should be explored. In cryptozoology, this involves considering any other known animals or less-fantastic phenomena that may account for the reported evidence; regarding the Agnagna report, this means considering whether any animals beside a sauropod could account for this reported sighting of Mokele-Mbembe.

In his report, Agnagna does not actually state that he saw a sauropod.

However, his statement "it can be said with certainty that the animal we saw was Mokele-Mbembe," seems to imply that he believes he definitely saw a sauropod. In fact, the Winter, 1983, issue of *The ISC Newsletter* (Vol. 2[4]) reports that, in a personal communication to Roy Makal, Agnagna flatly stated: "Mokele-Mbembe is a species of sauropod living in the Likouala swamps and rivers. I saw the animal."

I do not question the integrity of Agnagna or the other 1983 expedition members, but even scientists are quite fallible when it comes to describing unfamiliar events or phenomena, especially when such events are witnessed under less than ideal conditions. Regarding the 1983 Lake Telle sighting, we seem to have a set of circumstances that leaves room for doubt as to whether the "eyewitnesses" can be sure, let alone whether we can be sure, that they actually saw a sauropod:

1. The event was of relatively short duration.
2. The object was seen at a considerable distance (an estimated 240–300 meters).
3. Only the back, neck, and head of the animal was seen.
4. The animal was submerged in—and surrounded by—water, making judgement of size difficult for lack of a reference scale.

Under these circumstances, we must at least be cautious about accepting that the creature was a sauropod, whether or not it may represent what the natives call "Mokele-Mbembe." Many students of psychology are familiar with the classroom experiment in which a teacher arranges for an unexpected intruder to enter the room, perform some action, and then hurriedly leave. The students are asked to describe what they saw. Very often, the descriptions differ considerably from what actually took place, especially in regard to details. And this involves an event seen at a distance of only a few feet! The viewers' mistaken perceptions may at least partially be due to their preconceptions and to their heightened emotions. We may suppose that some of both were present during the reported Mokele-Mbembe sighting. In fact, the emotional factor seems also to have had at least one unfortunate result, as Agnagna relates, "the emotion and alarm at this sudden, unexpected event disrupted the authors attempt to film the animal . . ." The distance from which the creature was observed, and the fact that it was submerged in and surrounded by water, also raise strong questions in my mind as to whether the size or features of the animal could be accurately or reliably judged.

Despite these considerations, I do not completely discount the reported description, and believe it is reasonable to assume that the witnesses did see the upper part of an animal which corresponds, at least roughly, to the given description (small head, long neck, wide back). But let us consider what other creatures might present a similar profile. Two animals that come to mind immediately are a large turtle and a large snake, as shown in Fig. 1. Both are possible inhabitants of Lake Telle or other Congolese lakes and swamps.

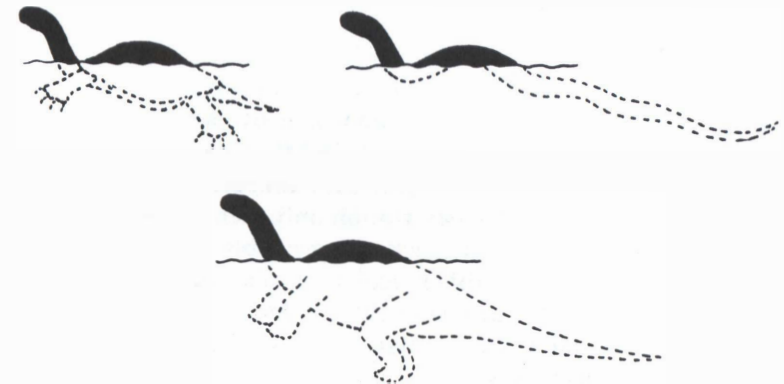


Fig. 1.—Hypothetical profiles of three possible Mokele-Mbembe animals rising above the surface of the water—a turtle, a snake, and a sauropod.

The large turtle hypothesis seems much more likely as a possible explanation of the Lake Telle sighting. First, the shell of a large turtle would account for the "wide back" feature. The head and neck, if from a large, long necked turtle, might look remarkably like that of a sauropod, especially from a distance. Such a hypothesis begins to seem even more plausible when one considers that Agnagna reports that there were frequent sightings of an enormous turtle (with a shell of 2 meters!) swimming in Lake Telle.

The large estimated size of the Mokele-Mbembe creature (5 meters) does not rule out the turtle hypothesis, since the reported size of the Mokele-Mbembe creature seen can only be a rough guess at best. In fact, considering the reported depth of Lake Telle (2–3 meters) the turtle hypothesis again seems much more plausible than the sauropod hypothesis. I find it difficult to imagine that even one 5-meter sauropod (let alone a population of sauropods) could live in a lake 2–3 meters deep without making constant disturbances and appearances. How could it walk (or swim) without making a wake? A turtle 2 meters long, however, is large enough to explain the sighting, and, because of its low, streamlined body and aquatic lifestyle, it could more easily swim and stay underwater for long periods without disturbing the surface (perhaps explaining why only once during the expedition's stay at Lake Telle was "Mokele-Mbembe" seen above water). I am not stating that the reported sighting of Mokele-Mbembe was definitely a turtle, but I do think we should consider that there may be animals other than a sauropod that might account for this sighting.

Whether a large turtle might account for the Mokele-Mbembe legend in general is another question. I might suggest, however, that even the basic form of the entire body of a large turtle or tortoise might resemble the general outline of a sauropod (reptilian head, four clawed feet, reptilian tail). A local

native might associate a picture of a sauropod with the name of an animal it even vaguely resembles, and with which he is familiar (such as a large turtle). This possibility could be explored by asking the natives to identify pictures of both large turtles and sauropods, or by having the natives themselves sketch the creature they call Mokele-Mbembe.

Regarding the sketch drawn by Agnagna, I am curious about why the head is shown in white, whereas the description only mentions the creature being brown, with the back part of the neck appearing black. What does the white face represent if not a color difference? I am also curious about what the vertical line drawn on the muzzle of the creature represents.

Last, considering that hunter Immanuel Mongoumela claims to have seen the creature and its prints several times, why not provide him with a camera and film so that he can document his sightings? This would seem a rather promising and inexpensive way of gaining some verifiable evidence of the creature's existence. In fact, why not provide the local people with cameras and instructions on their use, with perhaps some reward or incentive for capturing the creature on film? Even this would seem inexpensive compared with the cost of a full-scale expedition from the United States.

I am not dismissing or attempting to refute the 1983 Mokele-Mbembe report. I am only suggesting that the recently reported sightings are open to alternate explanations and interpretations, and I wish any future expeditions success in obtaining incontrovertible evidence of the true nature of the Mokele-Mbembe animal.

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NEITHER TURTLE NOR SNAKE

(Response to Kuban)

I would like to make the following observations concerning Kuban's Comment:

1) The animal I saw in Lake Telle could not have been a turtle or a snake, for reasons I give below.

2) The soundings we took in the area where the animal was seen indicated a depth of more than 6 meters, even though the mean depth of the lake is about 2.5 meters. There are several areas of the lake where the depth is greater than 6 meters.

3) There is an arm of the lake which flows right to the spot where the animal was observed; this arm has a depth of about 4–5 meters right up to where it is difficult to continue because of dense vegetation.

4) Leaves of Mabodji had been eaten in that spot, to a great height. A herbivore that could have reached that height from the water, which had a depth of 4 meters, would have had to have been of considerable size.

5) The animal I observed could not have been a turtle because of the proportion of the size of the neck in relation to the rest of the body. Furthermore, turtles do not surface with their necks extended in such a manner.

6) The animal could not have been a snake. No known snake can elevate itself above the surface of the water for a meter or more.

It is indeed unfortunate that I cannot at this time produce a photograph of the animal I observed.

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THE SAUROPOD HYPOTHESIS: AN EVALUATION OF THE CONGOLESE REPORT ON MOKELE-MBEMBE

(Comment on Agnagna, *Cryptozoology*, Vol. 2: 103–12)

When I showed a copy of Agnagna's report to my colleagues at Brown University, I received the following responses (which were by no means mutually exclusive):

1) It must be a hoax, probably to drum up tourist trade.

2) It would be biologically impossible for a small localized population of large animals to survive for 60 odd million years.

3) You really *want* there to be a surviving dinosaur, don't you? (Vertebrate evolution is one of the courses that I teach at Brown.)

It is to the credit of my colleagues, most of them ecologists and population biologists, that they made little comment on the lack of photographic evidence. As field biologists, they are well aware of how difficult it is to obtain good pictures of animals that are known to occur even in moderate abundance in areas of clear visibility. (A good example of this would be cheetahs in East Africa, which are rarely even seen by tourists.) However, I intend

to phrase my comments on this paper as a response to these criticisms of the possible existence of Mokele-Mbembe.

First: Could the whole thing have been a hoax? This is, of course, a possibility, although there have been local stories about such an animal long before the publication of this report. If so, what would the motive be? It could hardly be to drum up tourist trade, as the area in which the animal is reportedly present is highly inaccessible, particularly for tourists requiring Western comforts. Even in the case of the Loch Ness Monster, which supposedly resides in a lake that is highly accessible to the public, and is close to a city, as a visitor to the Inverness area I can testify to the fact that there is relatively little specific "Nessie" tourist trade.

I suppose that a hoax could be perpetrated to draw attention to the exploits of an individual scientist, but the impression that I have received from reading the article by Agnagna, and the interview with him in *The ISC Newsletter* (Vol. 3[2]), does not strike me this way. My own feeling is that, if someone were going to perpetrate a hoax such as this, he or she would do a better job of faking photographic evidence, but this opinion could just reflect my Western bias.

If Agnagna is *not* perpetrating a hoax, could he be mistaken in what he saw? This is also a possibility; perhaps the so-called sauropod could be explained away as some local animal seen from an unusual angle, in poor light conditions, etc. etc. However, for my money, it is hard to beat the combination of qualities possessed by Agnagna: that of a local person, so familiar with the range of animals known to be present in the area, who has also been trained as a biologist, and so, hopefully, as capable of making as clear and unbiased description of an unknown animal as could be expected by any observer. This is not to say that I would "believe" every word of Agnagna. The question is: given his expertise and training, and also assuming that the whole thing is not a hoax, what is the probability of his being seriously mistaken about this supposed unknown animal? As a scientist, I like to think that, within the framework of scientific thought, there is room for shades of opinion between complete acceptance of a hypothesis and its outright rejection. After all, science is not supposed to be a belief system.

If there *is* some undescribed large animal in the Likouala swamps, what is the likelihood that it could be a surviving sauropod—presumably one of a small, surviving population? I do not consider this to be highly probable. But, given the fact that the Congolese swamp forests and vegetation have been virtually undisturbed—at least in terms of latitudinal position in the equatorial zone—since the end of the Cretaceous, the fact that sauropods survived in this region at least up until this time, and the fact that there has been no fossil record available from the Central African equatorial region since the end of the Cretaceous, I am forced to conclude that, if there was anywhere on earth where sauropods *might* have survived until Recent times,

this would be the most likely place. (Sauropods also survived in South America until the end of the Cretaceous, but there *is* a meager Cenozoic record from the South American equatorial regions, and it shows no evidence of sauropod remains.)

To return to the second criticism of my colleagues, would it have been possible for a small population of sauropods to have survived in a localized area for such a long period of time (65 million years)? One comment I received was: "Look at the damage that elephants do to their habitat in Africa! A dinosaur would be five times that size; you couldn't sustain a small local population of animals that big." The answer to this point must relate to whether these supposed sauropods are ectothermic ("cold-blooded") or endothermic ("warm-blooded") in their physiology, regardless of whether or not one thinks that *other* dinosaurs may have been endothermic. (My personal opinion on the subject of dinosaur physiology is that there was probably a wide range of physiologies amongst different dinosaurs. Sauropods are the most likely candidates for ectothermy, not only because of their enormous bulk, but also because of their pattern of geographic distribution. They were found world-wide in the Jurassic period, when the continents were united in a single land mass. But in the Cretaceous, when the continents began to drift apart, presumably resulting in a more seasonal type of climate in the land masses in higher latitudes, they became restricted to the equatorial regions of the southern continents, mimicing the present day distribution of large-bodied ectotherms.)

Sauropods have been estimated as having body weights ranging between 15 and 50 tons, depending on the species. A 20-ton sauropod, with a similar metabolic rate to a 4–5 ton elephant, might well be expected to have enormous daily requirements of vegetation, and to require a very extensive home range to support a small population, with the expectation of considerable habitat damage that would be readily visible to investigators. However, an *ectothermic*, 20-ton sauropod would only have a similar daily requirement of vegetation to a 2-ton hippo. Small localized populations of hippos survive today in African rivers and lakes without causing extensive habitat damage. It is possible that a small population of ectothermic sauropods could maintain itself indefinitely within the confines of the Likouala swamps, though I would have doubts about endothermic sauropods.

Could such a population have survived and maintained itself in a localized area for many millions of years? What we know from population biology would tell us that this is highly unlikely; but no amount of mathematical modelling could tell us that this is categorically impossible.

Finally: do I really *want* there to be a surviving sauropod in the Congo? Well, it would be nice, wouldn't it?! But I consider my "biased" response to this question to be far less emotional and more scientific than the response of: "It can't possibly be true, so why even consider it?" To sum up: The

whole thing *may* be a hoax. If not, the animal *may* turn out to be something familiar; and even if it is an as-yet undescribed animal, the chances of it being a surviving sauropod are slim—but not outside the bounds of possibility.

On the basis of the presently available evidence, I do not think that I can say anything more than this as to the pros and cons of the possibility of the existence of an extant sauropod in the Congo. Nor, for that matter, could anybody else.

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SEMANTICS VERSUS CRYPTOZOOLOGY

(Comment on Wagner et al., *Cryptozoology*, Vol. 2: 113–25)

Everyone knows that mermaids are elusive creatures—as slippery to the hand as to the eye. Roy Wagner and colleagues have again treated us to a tantalizing glimpse into that zone of human endeavour where science attempts to interpret folklore. In June, 1983, a small group of cryptozoologists set out for New Ireland to further investigate the question of the identity of the Ri, semimythical animals known to some natives in that part of the world and said to resemble mermaids. The group interviewed several witnesses, and actually managed to observe and to photograph animals of some kind.

As a zoologist, I must reluctantly conclude that I know very little more about the zoological identity of the Ri now than before I read the Field Report by Wagner and his colleagues. Observations were made under sub-optimal conditions, and clear, detailed descriptions of the Ri were not possible. Two witnesses to an incident in which an animal reported to be a Ri was killed and eaten affirmed that it was indeed a Ri; a third remembered it being a dugong. Searches for Ri bones in village bone heaps only revealed bones of pigs. The photographs, taken under difficult conditions, are of poor quality, and show only a blurry hump, identified as a Ri rolling at the surface after a dive, and an equally blurry object, plausibly resembling the flukes of some marine mammal.

The descriptions of Ri behaviour are more substantial, and are really the most zoologically valuable material in the report. Ri dive with a periodicity of about 10 minutes, which is apparently much longer than dugongs dive.

The “hump” made as the Ri surfaces is reported to be much sharper than that observed for dugongs. Ri clearly have flukes. Small fish scatter in flight from the Ri, an observation interpreted by Wagner et al. as indicating that Ri are predators. These observations, and the ambiguous testimony of witnesses, lead Wagner and colleagues to the conclusion that the Ri are not dugongs.

I find it difficult to share this conclusion. The natural history of sirenians in general and dugongs in particular is not well known. Is it not possible for their behaviour to vary between Queensland and Northeastern Papua New Guinea? The perceived shape of the hump must depend strongly on the angle from which the animal is viewed. Small fish scatter when *any* large shape moves swiftly in their direction. As the authors themselves are quick to point out, there is nothing in their observations to unambiguously eliminate dugongs from being the prime candidate for the zoological identity of Ri. Clearly more evidence is required, a conclusion I do share with Wagner and his coinvestigators.

In gathering additional evidence, future investigators should bear in mind that the question belongs perhaps as much in the realms of semantics or linguistics as in the realm of zoology. This assertion is not meant to diminish the importance of the problem, but rather to enlarge it. By Wagner's own account, people from different villages or regions in New Ireland may a) not recognize Ri (or at least have no word for it), b) recognize Ri and call it that, or c) recognize an animal like the Ri but call it Ilkai. All apparently recognize dugongs and dolphins. This area of the world is notorious for the diversity of its indigenous languages. The people interviewed by Wagner are not English speakers, nor is Pidgin their mother tongue. It is likely that each region, and perhaps each village, may have its own distinct language.

What relevance does this situation have to the cryptozoologist? It is well established, I think, that the vocabulary and grammar of language determine both the content and mode of thought in subtle ways. Could it be that Ri and Ilkai and their kin are products of what might be called a cultural semantic—that they are appellations for things not clearly seen, yet uniquely named and given tutelary or other significance? If this is the case, one can easily imagine the semantic and cultural difficulties faced when such an animal is occasionally seen clearly or even captured and killed.

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IS THE RI A PORPOISE?

(Comment on Wagner, *Cryptozoology*, Vol. 1: 33–39, and Wagner et al., *Cryptozoology*, Vol. 2: 113–25)

We have seen two Comments in a previous issue of this journal on Wagner's first article (see John R. Sibert, 1983, *Ri-ality: A Mermaid in the Hand?*, *Cryptozoology*, Vol. 2: 159–61; see also James G. Mead, 1983, *Ri or Dugong?* *Cryptozoology*, Vol. 2: 161–62). Both discussed the possibility of the Ri being some sort of unknown marine mammal. Mead, a recognized authority on marine mammals, stated: "The author gives enough information about the habits and appearance of the Ri to satisfy me that it is not a porpoise."

In re-reading Wagner, and the follow-up Field Report by Wagner et al., I am not so sure. While there are many references to "long head hair" and "mammalian breasts," these come only from heresay reports, and when the 1983 expedition to New Ireland actually saw what they believed to be a Ri, they produced descriptions that could easily be applied to a porpoise (pp. 118–20).

I am not qualified to discuss the anthropological implications of the Ri sightings in New Ireland, or their place in local mythology, but I do have a suggestion that might prove useful in the analysis of these sightings. Known to inhabit these waters is a small *finless* porpoise, *Neophocaena phocaenoides*. (Its common name in English derives from its *dorsal-finless* appearance; it has the requisite pectoral and caudal structures.) *Neophocaena* is a grayish porpoise that is found in shallow inshore waters from Iran all the way around India and Sri Lanka, throughout the islands of Southeast Asia to New Guinea, and as far north as the Inland Sea of Japan. It has also been reported from the Yangtze (Changjiang) River, and from Tung Ting Lake in China. Since there are no specimens to work from, we still have no good idea of the true appearance of the Ri, but, with one problematical exception, its behavior seems consistent with that of a small porpoise. It has been seen to roll slowly in the water, occasionally "throwing its flukes" in the air as it dives.

Neophocaena is a member of the family Phocoenidae, which includes the harbor porpoise (*Phocoena phocoena*), the vaquita (*Phocoena sinus*), Burmeister's porpoise (*Phocoena spinipinnis*), the spectacled porpoise (*Phocoena dioptrica*), and the Dall porpoise (*Phocoenoides dalli*). With the exception of the Dall porpoise, which is a pelagic North Pacific species, all the others are found in shallow inshore waters. Furthermore, they are usually shy, and stay away from people and boats. I am relatively familiar with the harbor porpoise (*Phocoena phocoena*), having observed this species in the wild in Alaskan and New England waters. Perhaps the most remarkable aspect of the behavior of these small cetaceans is their ability to "disappear" after having been sighted and identified. In glass-calm waters, I have seen these

porpoises surface, blow, and then submerge, *never* to be seen again by the observers.

This brings us to the problematical aspect of the 1983 observations of the Ri: their reported ten-minute dive times. None of the small porpoises are known to remain submerged for anything approximating ten minutes. One authority gives "45–75 seconds" as the average dive time for *Neophocaena* (see Lyle Watson, 1981, *Sea Guide to Whales of the World*, E. P. Dutton, New York). It is possible, however, that the intermediate dives—that is, those *between* these ten-minute sightings—were missed by the observers. An animal without a dorsal fin is more likely to be missed on a shallow surfacing than one with a prominent fin. In the waters of Golfo San Jose, Argentina, Würsig, Würsig, and Mermoz reported that Burmeister's porpoise (*P. spinipinnis*) "was difficult to see, even in calm waters, because of their typical phocoenid behavior of gently rolling to the surface, exposing only the blow-hole and the dorsal fin" (see Richard Ellis, 1982, *Dolphins and Porpoises*, Alfred A. Knopf, New York).

None of this explains the "long dark head hair," "mammalian breasts," or human-like genitals. (It is not clear from Wagner's references to "eyewitness accounts" whether or not the genitals protruded; if they did, they would render this animal most unusual for a marine mammal.) Until the collection of an actual specimen, I think the possibility that the Ri might be a finless porpoise should not be dismissed too quickly.

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IS THE RI A TROPICAL BELUGA?

(Comment on Wagner et al., *Cryptozoology*, Vol. 2: 113–25)

In this article, the authors give details of observations made on a marine, probably mammalian organism known to the natives of New Ireland as the Ri or Ilkai. By comparing their observations with published data, they conclude that the animal is not definitely identifiable as a dugong (*Dugong dugon*), southern right whale dolphin (*Lissodelphis peroni*), or finless porpoise (*Neophocaena phocaenoides*). These were the only three species which they specifically considered, since they are almost the only mammals sharing the characteristics of a crescent-shaped tail and absence of a dorsal fin with the animal observed. However, there is another cetacean which has these

features and, rather surprisingly, may deserve to be considered as a possible Ri.

The beluga or white whale (*Delphinapterus leucas*) was presumably discounted by Wagner et al. on three major counts: color, size, and distribution. Neither the white color nor the 13–14-foot length of the adult seem compatible with the dark color and 5–7-foot size (stated in *The ISC Newsletter*, Vol. 2[2], but not in the above report) of the animal seen by the expedition. Furthermore, the accepted southern limit of the species' range is about 40°N, well over 3,000 miles from this reported sighting. Nevertheless, these factors are not enough to eliminate this species, or more accurately, a close relative, from consideration.

In the British Museum (Natural History) is a 38-centimeter-long beluga skull, accession number 368a, to which two labels are attached. The original label reads: "368a, Type, *Beluga kingii*, Gray, Zool. of the Erebus & Terror, p. 30, New Holland, Capn. P. P. King," whereas the second, much later label states: "*Delphinapterus leucas*. Type of *Delphinus* (*Delphinapterus*) *kingii*, Gray, Phil. Mag.n.s.II, Nov 1827, 375, Pres. Capt. P. P. King R.N. Locality: said to come from coast of Australia." No other data concerning the history of this skull are available, as King himself makes no mention of either a white whale or the skull in his account of the voyage (see Hershkovitz, 1966, *Bulletin of the U.S. National Museum*, Vol. 246: 1–259).

This skull provides the sole piece of evidence for the supposed species *Delphinapterus kingii*, and it is worth inquiring how well-founded this species is. Gray seems to have regarded the locality as a prime factor in his diagnosis, although he also listed a number of skull characteristics which differed from the other material at his disposal. However, by the time True reexamined the specimen in 1889, a much clearer idea of the extent of individual variation was available, and all the skull characteristics Gray had found could be matched by an Alaskan specimen in the U.S. National Collection (see True, 1889, *Bulletin of the U.S. National Museum*, Vol. 36: 146–47). Thanks to this paper, *D. kingii* was struck from the annals of zoology.

But what if the dubious locality were correct? Certainly a beluga-like whale could explain many of the reported features of the Ri, including both sighting data and native reports. To take the sighting first, the animal's color and size could be matched by a white whale calf, since at birth they are dark in color and around 4 feet long (see Lyall Watson, 1981, *Sea Guide to Whales of the World*, Dutton, New York), while the British Museum (Natural History) specimen belonged to a 10-foot individual. Also, the pattern of regular, 10-minute dives is well within the beluga's capabilities, as they normally dive for 5–15 minutes between breaths. Lastly, like the observed animal, the beluga feeds in shallow water.

When it comes to the native descriptions, the best point of similarity is the protruding mouth. The beluga has a short beak, but the presence of a

forehead melon also gives it a quite human look, and it is renowned for its range of facial expressions (see Brodie, p. 203, in Macdonald, 1984, *The Encyclopaedia of Mammals*, Vol. 1, George Allen & Unwin, London). This, coupled with unfused neck vertebrae permitting a high degree of mobility, results in an acceptably mermaid-like head and neck. The beluga is also an extremely vocal animal, an attribute commemorated in the alternative name of "sea canary." The whistling sound which they make has its counterpart in the sound reported by at least one New Irelander (see Wagner, 1982, *The ri—Unidentified Aquatic Animals of New Ireland*, Papua New Guinea, *Cryptozoology*, Vol. 1: 33–39). Finally, the Ri has been seen in groups of up to six, while the beluga lives in pods of up to 10 or so, suggesting a similar degree of sociality.

However, not all the reported features of the Ri are shared with *Delphinapterus*. For example, Wagner describes the animal he saw as "slender," while the beluga is rather plump. Similarly, the swift motion and visible back of the Ri contrast with the slow swimming motion and rarely visible back of the white whale observed in the Rhine by Gewalt (see *Grzimek's Animal Life Encyclopaedia*, 1975, Vol. 11, p. 504, Van Nostrand Reinhold, New York); and the beluga's "gentle undulations" (see Watson, 1981, above), and the Ri's "extreme vertical flexure" scarcely seem the same. The picture becomes more confused if we try to account for more of the features in native descriptions, such as the arms fused to the side of the body, and the long hair on the head. Here I feel forced to take the "establishment" view, and regard these as unproven until verified by Western (or Westernized) observers.

Thus, after reviewing the available evidence, I feel that there is a possibility that the twin mysteries of the Ri and *Delphinapterus kingii* may be interconnected, and that the hypothesis that they are one and the same organism should be given due consideration.

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THE NATURE OF THE RI

(Response to Sibert, Ellis, and Britton)

It is a pleasure to respond to careful, educated, and informed commentary by experts in marine biology on our speculations as to the nature of the Ri

(Ilkai). Generally, cryptozoological speculation often comes down to a sort of tug of war between those who seek approbation for having a clear, unfettered outlook, and those who wish special recognition for a disciplined and rigorous approach. In these three cases, however, some very specific candidates for the identity of the Ri have been put forward, and they deserve careful consideration. It is tautological, but nevertheless necessary, to point out that we have *not* argued for the native interpretation of the animal's characteristics, though it is necessary, in the interest of fairness, to add that we have not argued strenuously against it either. The case is open, as it should be, and what is most needed is further evidence—an old story, to be sure, in cryptozoology.

Sibert, who revives the case for the dugong, identifies an important factor in what is known about the Ri—that our very reasons for suspecting something unusual here all result from what we were told by native New Irelanders. No sightings, indeed, were made of the creature except through the mediation, in one way or another, of these natives. I would not agree, however, that Melanesian Pidgin is not, by this time in this century, a mother tongue of these people: it is learned concurrently with Barok or Susurunga from early childhood, and it is, for historical reasons, closely cognate with those Austronesian languages. Certainly, Ri are products of a cultural semantic—so, in a number of ways, are human beings; certainly the semantic and cultural difficulties raised when one of the creatures is encountered are considerable, and for this reason Sibert could find many New Irelanders who would argue that the Ri is a dugong, as we found many who did. But a Barok native who captured a dugong would most likely call it *a bo nara si*, and not add that it was a *tori* (“man”) or call it *man* (Pidgin: “human being”), and Barok do not claim that they capture other semantic products like *gilam*, *gas*, or *mawus lie*.

Ellis calls our attention to the finless porpoise, *Neophocaena phocaenoides*, whose diving behavior certainly matches that recorded in one of my poor photographs. At least in the behavioral context, this makes some sense. Here, however, one would want to call attention to the acute flexure of the Ri while rolling at the surface, and also to its long (10 minute) dives. I am reasonably certain, as one who kept the inshore bay area under extremely close surveillance for something like two hours, that neither I nor the natives watching with me would have missed additional surfacings of the creature; as it was, the 10 minute intervals were remarkably consistent throughout. I very much agree with Ellis, however, that the finless porpoise, and indeed several other likely candidates, should not be dismissed too quickly.

Britton raises the possibility of the beluga, or white whale, and also notes the importance of color. Just because our data are so sparse, I tend to regard skin color as a rather important bit of evidence, as the sighting I made in full sunlight at Ramat Bay showed a rather strikingly *light* green shade. A

native of Watom Island, outside of Rabaul, New Britain, told Kurt Von Nieda and I that the creature known to his people has a skin color like “a ripe mango”; that is, light green and orange. Likewise, Kaminie Kurus, who was present with Richard Greenwell and I in the boat at Nokon, claimed he had seen the “nice white face” of the Ri in the water as we were approaching. Were I forced to make a choice among the *known* sea mammals as a candidate for the Ri, I would certainly opt for a small beluga whale as described by Britton.

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THE RI: ZOOLOGY AND FOLKLORE

(Response to Sibert, Ellis, and Britton)

While it is interesting to try to determine what known animal—if any—the Ri (Ilkai) might be, it is also interesting to observe how different commentators propose different animal candidates with varying degrees of certainty. Obviously, only one, at most, can be correct, at least for the animal actually observed by the Wagner team.

Sibert raises the possibility of the dugong once again, which was addressed previously by Mead (see James G. Mead, 1983, Ri or Dugong?, *Cryptozoology*, Vol. 1: 161–62). Indeed, one would expect this to be the most likely candidate, but we have serious doubts about this hypothesis, at least in terms of the Ri we saw—assuming that the animal pointed out to us on several occasions by native inhabitants was the same Ri which they conceptualize. (A strong argument in favor of this was my little experiment in which I excitedly pointed to two easily-identifiable dolphins which had entered Nokon bay, exclaiming the Susurunga word “Ilkai,” only to be informed that I was mistaken, that the Ilkai was different.) The specific reasons why we doubt the dugong hypothesis are given in our previous report.

Ellis makes a strong case for the finless porpoise, *Neophocaena phocaenoides*. This possibility was also addressed in our report, and we remain dubious. Ellis states that the range of *Neophocaena* extends to New Guinea, but cites no source for this information. This, however, is not a point we would strongly debate, as such a range extension is certainly possible. Concerning diving times, Ellis has a good point; although we doubt it, maybe we *did* miss many surfacings, making it only *appear* that the animal surfaced

every 10 minutes. Two problems arise with this explanation, however, which Ellis, or other knowledgeable persons, should address: 1) The animal kept surfacing at almost exactly the same spot, decreasing the probability of a "miss," as we knew were to observe, and the bay's surface was calm at the time. 2) If we missed "in-between" surfacings, as Ellis reasonably suggests, why did we consistently only catch surfacings every 10 minutes? If we were, in fact, missing most of the surfacings, one would expect us to have caught the ones we did in a random fashion, after, say, 4 minutes, 13 minutes, 5 minutes, 9 minutes, etc. However, we consistently observed it rolling almost exactly every 10 minutes for about a two-hour period.

Perhaps the surfacings we missed, if Ellis' suggestion is correct, were not rolls, but more surreptitious surfacings. However, this still does not explain why the roll-surfacings consistently occurred every 10 minutes.

In addition to the above, there is the problem of *Neophocaena's* bulky body, which, in our opinion, makes it an unlikely candidate—and this is equally so for the dugong. The vertical flexure of the animal we observed was discussed in our previous report, but Ellis does not address this issue.

Having stated all this, however, I should add that my personal, subjective impression of the animal when we were within 50 feet of it—and I have no objective evidence to support that impression—is that it was a small cetacean of some kind. If I were to have to choose one of the candidates we and others have discussed, I would choose *Neophocaena*. So Ellis may be on the right track.

Britton's discussion of the beluga *Delphinapterus kingii*, which "was struck from the annals of zoology," is intriguing, particularly when one considers the morphological features he mentions. It is tempting to "interconnect" these two mysteries, as Britton suggests. However, the bulkiness of the proposed beluga again makes it an unlikely candidate, as the animal we saw was able to roll rather sharply at the surface. Britton himself mentions this problem, among others, but he is to be congratulated for producing another serious candidate for consideration.

Sibert's discussion of the semantic nature of the Ri/Ilkai, and the relationship between language and thought, raise some other points I would like to address here. They concern not so much the problem of native perceptions, but more the problem of some Western perceptions. It has been suggested by various observers, one writing in *The Skeptical Inquirer* for example, that we were seeking the literal mermaid, presumably as portrayed in Western art and literature. This is a very simplistic view, but it provides an excellent opportunity to ridicule an interesting folkloric and zoological question.

For centuries, there has been discussion as to what animals may have been the bases for mermaid legends. Manatees and dugongs have been the principal candidates, but scholars could only speculate. Bernard Heuvelmans

has already pointed out in this journal how animals become mythified, and he uses the unicorn as an example:

... sometimes the degree of mythification is such that the original species is no longer recognizable, unless one takes the trouble to trace the source of the legend. This is the case, for instance, with the unicorn, which finally acquired an existence quite distinct from the animal which "gave birth" to it. The unicorn is the final product of a series of transformations undergone by the Indian rhinoceros to get it to conform to the myth of aggressive virility (see Bernard Heuvelmans, 1982, What is Cryptozoology?, *Cryptozoology*, Vol. 1: 1-12).

With the Ri, there was an excellent opportunity to short-cut the "tracing back" task outlined above, and observe the living animal itself to determine the origin of the myth. There was also the possibility that the animal could be new to zoology—and this possibility remains.

While I do not necessarily speak for all the Wagner team members, I think that, interestingly, the dugong is not the basis for this particular mermaid myth (again, assuming that the animal pointed out to us was the same animal responsible for the myth), but that a small cetacean was—this does not, of course, rule out the possibility of the dugong having played a role somehow in the propagation of the myth. In this connection, I was interested to learn that explorer Jacques Cousteau, during his recent incursion into the Amazon Basin, found that the natives give human attributes to the freshwater Amazon dolphin, not the freshwater Amazon manatee.

There is another possibility concerning the source of the mermaid myth that should be addressed. While not believing in literal mermaids (which portray an impossible mammal-fish hybridization), some observers wonder about the possible existence of an aquatic ape. While the idea may seem utterly absurd, there are no fundamental biological reasons to rule this out. Several other groups of terrestrial mammals—quite unrelated—have representatives that "returned" to an aquatic existence; so why not one more, the primates?

There are good reasons for seriously doubting this possibility. Both monkeys and apes are basically arboreal. Even the so-called "ground-dwelling" primates are intimately linked to the trees, both in morphology and—usually—behavior; almost all retreat to them when in danger. There should never have been any need to "retreat" to the water, or to exploit its resources, as some other ground-dwelling mammals did.

In fact, only a fully ground-adapted hominid—one of the erect "early men"—could have needed such a haven or area to exploit, but, by then, hominids had begun exploiting other advantages for terrestrial living: increased brain volume, tool/weapon use, etc.

In view of these evolutionary factors, the case for the aquatic ape seems very dismal indeed. The only evidence to support it is the sighting of a "sea

ape" by Georg Wilhelm Steller on August 10, 1741, in the Gulf of Alaska, and present-day speculations fueled, in part, by the writings of Elaine Morgan, author of *The Aquatic Ape*.

Hopefully, observers will return to Nokon bay one day, and determine exactly what kind of animal the Ri/Ilkai is, which will solve the problem once and for all. Such an expedition should have at its disposal equipment such as sonar, in order to know exactly where the animal is and when it will surface, as well as better photographic and/or video equipment. If the animal turns out to be a known one, particularly a dugong, it will have served as a valuable lesson to the folklorist, the historian, and, of course, the cryptozoologist.

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Ri EVIDENCE LACKING

(Comment on Wagner, *Cryptozoology*, Vol. 1: 33-39, and Wagner et al., *Cryptozoology*, Vol. 2: 113-25)

Finding Wagner's first article on the Ri intriguing, I visited New Ireland in early June, 1983. Prior to leaving, Wagner gave me names of contacts in Namatanai and Ramat Bay. I spent the period June 8-18 in the Namatanai-Ramat Bay area, and while there, I discovered that the main informant who had been seeing the mermaid-like Ri in Ramat Bay was now dead. Through interpreters, I queried villagers in Ramat village, Pire village, several other villages on the edge of Ramat Bay, in Namatanai village close by the town of Namatanai, and in Punam village and Bo village, both south of Namatanai. I also queried students at Namatanai High School, and their teachers. I offered a substantial reward for physical evidence of the mermaid-like Ri, and I also offered a smaller reward just to be able to see a dugong.

My interviews suggest that, after having talked to over one hundred natives about the Ri and about dugongs: 1) only in Pire and Ramat villages did the natives use the term Ri to signify a mermaid-like creature, and that the villagers in Bo, Punam, and Namatanai used the term to signify the dugong. 2) Common useage throughout New Ireland, according to those villagers I queried, was that Ri meant dugong, and that only in the Ramat Bay area had it become related to mermaid-like creatures. 3) No natives in any village, anywhere I asked, would verify that they had told Wagner that they or anyone else had ever eaten a mermaid-like creature at any time. Contrary to Wagner's report, the men of Namatanai village reported that they had kept a dugong,

not a mermaid, in their men's hut for a week before they ate it. (I did observe that they also kept live sea turtles in their village for some time before they would eat them at a feast.) 4) The magistrate, a native who Wagner had reported had thrown his spear at a Ri (henceforth in the article, Ri will be used to signify the type of creature reported by Ramat and Pire villagers, i.e., a mermaid-like creature) told me he had thrown his spear at a dugong. In ten days of searching and questioning, I could not find any native who would admit to either catching and butchering a Ri, nor even of telling Wagner that they had done so. I was able to locate several men who told me of catching an animal in a net, as Wagner had described, except that they denied catching a Ri, as Wagner had reported; they stated that it was a dugong, and that they took it to the market to sell. 5) When shown photographs of a dugong and a drawing of a mermaid, invariably the natives chose the dugong in respect to eating or catching them. When long distance sightings were discussed, I obtained ten reports of the Ri. Only in the Ramat Bay area did anyone claim to have close-up sightings, at high tide. 6) I obtained no physical evidence of either dugongs or Ri, nor was I able to observe either in person.

In conclusion, I found no physical evidence of the Ri on my trip, and I found some natives who would only verify Wagner's reports to the extent that they admitted having at one time or another captured and eaten a dugong. I came to feel that, while some natives had seen dugongs, most of them had little direct knowledge of dugongs, for these are now somewhat rare in the area due to man's predation.

In regard to the Ilkai/Ri sightings by Wagner and associates, I would like to point out that, while watching for Ri or dugongs, I noted that a short-finned pilot whale would surface every five minutes just off the reef barrier at Ramat Bay, and that it would stay up for just two seconds as it got air and dived again.

Finally, an article on the manatee, the dugong's close cousin, in the September, 1969, *National Geographic* (p. 350), points out that manatees sleep below the surface, that they rise slowly every five minutes or so to get air, while sleeping, and that they spend about four seconds only in getting air. One young manatee was observed to spend as long as 13 minutes between surfacings. While feeding, they surface more often, usually about every minute. If it is reasonable to assume that manatee behavior is close to dugong behavior, then perhaps the tan bodied Ri/Ilkai sighted by the 1983 expedition at Nokon and at Ramat Bay might simply have been a dugong that was sleeping in one instance, and then feeding in another instance.

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OBJECTIVE EVALUATION LACKING

(Response to Beckjord)

Beckjord is apparently aware, at least, that our knowledge—and even our observations—of the Ri was gained at the sufferance of the native peoples. Accurate communication is vital to *both*. What are Beckjord's credentials in this regard? He speaks no Barok, and to judge from the illegible handbills he plastered in public places in Namatanai and elsewhere, he could not make himself understood in Tokpisin, even with the aid of a translator. Thus, most of his allegations are unworthy of serious consideration. I should point out that, although Beckjord seems to be unaware of it, *ri* is a *Barok* word, while the people of Bo, Punam, and Namatanai villages are *not* Barok speakers. Also, the word *ri* is *not* in common usage throughout New Ireland.

Although we seriously doubt it, the possibility of the Ri—both as conceptualized by the natives and observed by us—being a dugong is a viable hypothesis. However, Beckjord's "conclusions" have more to do with preconceived notions and some particular motivations behind his sudden trip to New Ireland than to a careful and objective evaluation of the evidence.

ROY WAGNER

A SECOND LOOK AT THE PRESS' ATTITUDE TOWARD CRYPTOZOOLOGY

(Comment on Westrum, *Cryptozoology*, Vol. 2: 162–66).

Modern journalism's biggest handicap is that it is spread over too broad a range of communications vehicles. Its primary responsibility, to get the facts to the public, sometimes loses out to glitz and gobbledygook. Journalists used to be people who wrote articles for newspapers and magazines, who could be expected to follow some sort of code of ethics, and who had some knowledge of the language. Reporters who ignored that code, and editors who let them get away with it, were just bad journalists.

Now that the information business has exploded, communications technology is such that billions of people can find out about major world events hours—or minutes—after they occur. The immediacy of news presentation and its packaging have made viewers and readers quite sophisticated, not necessarily in their ability to assimilate the information given, but in their ability to discriminate between good presentation and poor presentation. The good news is that more people are aware of, and interested in, what's happening in the world. The bad news is that the fierce competition for their attention has given priority to presentation instead of content.

Everybody has now gotten into the information business, and few of the old rules have survived. Much of what goes on nowadays offends old-time newspapermen and English teachers. Westrum is justified in his impatience

with irresponsible journalism, and there is an inordinate amount these days. However, the market demanded massive and rapid changes, and, in fact, the loosening of the rules has helped such fields as cryptozoology a great deal. The information-gathering and reporting methods of a television show like "In Search of . . ." made reporters wince, but only in this way was it able to expose the public to so much cryptozoological information from scientists, eyewitnesses and, naturally, some wackos as well.

The reason that cryptozoological subjects don't get more and better exposure in the "higher echelons" of the press world is not the poor attitude of the press, but the nature of the subjects themselves. News reporting, by definition, revolves around news events. A scientific report on the potential food sources of Mokele-Mbembe is not news, but it would serve as good background if a Mokele-Mbembe is indeed produced. A sighting is news, but only if it is true, and even an entirely believable sighting does little more than add to the wealth of intriguing but useless nonevidence of the existence of supposed animals. A newspaper cannot be criticized for being suspicious of and/or selective in its reporting of cryptozoology-related items. And let's face it, the more selective a newspaper is in reporting the news, the more credence can be given to the news it *does* print—a sighting reported in the *National Enquirer*, whether it occurred or not, will not have the same believability as the same event reported in *The New York Times*.

Bauer, in noting that *The Times* of London has consistently downplayed Loch Ness sightings (see Henry Bauer, 1982, *The Loch Ness Monster: Public Perception and the Evidence*, *Cryptozoology*, Vol. 1: 40–45), stated that the paper "has not responded to the change which occurred among informed people. In part, no doubt, this has been due to the fact that the number of informed people is small and not well or readily known to journalists." In his Comment on Bauer, Westrum states: "When a newspaper person writes a report or an article about an odd event such as a Nessie sighting, scientific accuracy is seldom of concern." Both these statements show a lack of understanding of how the news should be reported. A sighting is a sighting, whether it's of Nessie, a Bigfoot, a UFO, Jimmy Hoffa, or Jesus' face in a windowpane. The journalist didn't see the object—someone else did. His job is not to prove that the object exists, or was even seen, but, rather, to inform the public that someone said he saw it. If there's a scientist handy to express an opinion, so much the better, but all the scientific pontificating in the world won't do much to substantiate the sighting itself. It's for the readers, or viewers, or listeners, to determine for themselves how much truth is behind the report. Respectable people believe in Nessie, but respectable people believe in ESP, and in God too. It's a different kind of material, a different kind of evidence, that the reporter must concern himself with.

Westrum was upset that a reporter ignored his carefully prepared remarks at the Manlike Monsters Conference at the University of British Columbia,

and instead printed some “semi-accurate” and “off-the-cuff” remarks he had made after the event. He’s in good company—a number of politicians, a “60 Minutes” correspondent and the President of the United States have also made inadvertent remarks in the presence of journalists, and were also astonished when they appeared in print. What was Westrum doing making “semi-accurate remarks” anyway? And if the reporter didn’t print his prepared remarks, should we automatically assume that the reporter’s news judgment was faulty? Could it be that the talk was just boring?

All journalists are continuously manipulated, and it irks them to no end. Usually, such manipulation is limited to requests for coverage by everyone from politicians to flower clubs, but sometimes people create events or lie about their experiences to get into the news. Cryptozoologists will probably agree that many people, although probably not the majority, claim to have seen something when they really haven’t. There have been so many sightings of some of these creatures that a sighting is easy to fabricate. When a journalist is suspicious, then, it’s not because he’s afraid of having his ego bruised by tittering colleagues; it’s because his profession demands that he report the facts.

Is it so unfair for journalists to want more tangible, physical evidence? This is the same position taken by many scientists, and most of the public as well. When Nessie swims up to a *National Geographic* photographer and visits for an hour or so, the number of believers among both scientists and journalists will skyrocket. But, for now, most members of “the press” and, I should think, most scientists would rather hear “I told you so” when such a delightful event finally occurs, than to professionally and publicly express beliefs that cannot be more tangibly backed up.

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A SECOND LOOK AT A SECOND LOOK

(Response to Smith)

One senses from Smith’s useful comments on the press and anomalies that she feels she has to defend how the press handles anomalies. Nonetheless, after reading her comments, and what I wrote originally, I see little reason

to modify my views. I would like to say one or two things, however, about what she states, although, for the most part, I agree with her. First of all, she sees eyewitness reports as “intriguing but useless nonevidence.” This statement shows little appreciation of how much useful information scientists can glean from eyewitness reports. Chladni’s identification of meteors and meteorites as belonging to the same physical phenomenon in 1794 was based on reading a large number of such “useless” reports. Chladni had never actually seen a meteorite when he wrote his book, now a classic in science. Such sightings may represent “nonevidence” to a reporter, but I doubt that many reporters could differentiate between a stony meteorite and any other kind of rock even if they could examine the evidence themselves. Reporters, as Smith points out, largely get their information from others—from scientists as well as observers. And for the most part, they are forced to accept society’s valuation of expertise or honesty. Few reporters have taken a course in forensic psychology. Contrary to what Smith says, however, the reporter is not a *tabula rasa* who simply reports. Reporters are expected to use judgment, and that determines both what gets presented and how it is presented. Good and bad reporters are such because of their use of judgment. The distortions on the “60 Minutes” television program are bad because they so often show misuse of judgment.

In an incident mentioned in my Comment, I related how a reporter used some “semi-accurate” off-the-cuff remarks I made, ignoring my prepared talk. I feel this was a mis-use of judgment since the rest of what I had to say, and which had been very carefully considered, was ignored, even though I felt its content to be important. I feel the reporter ignored what I had to say because it didn’t fit in with the “Bigfoot Follies” angle of his story. Smith suggests the talk was boring—actually, I was later told that it was one of the two best delivered during a very long meeting. She asks what right I had to make “semi-accurate remarks.” The answer is simply that I spoke without deep reflection. Reporters love carelessness because it often leads to more vivid copy. But greater insight? I wonder.

As to journalists being uninterested in the opinions of their colleagues and editors, and being interested simply in “reporting the facts,” I refer Smith to the references mentioned in my paper. Facts are usually socially constructed, and usually reflect the reporter’s judgment about whom one can believe, seldom what the reporter personally saw. The seasoned reporter, furthermore, knows exactly what kind of copy the editor wants and expects, and few reporters can get away with producing anything else.

Ultimately, the journalist who departs from professional conventions is rare. I don’t see this kind of conformity as irresponsible—it is a logical correlate of the structure of the community in which journalists operate. But it does have certain effects, which I sketched in my Comment, and will portray more fully in my forthcoming book on anomalies. Significantly,

Smith does not take issue with my contention that such distortions result from routine press practices.

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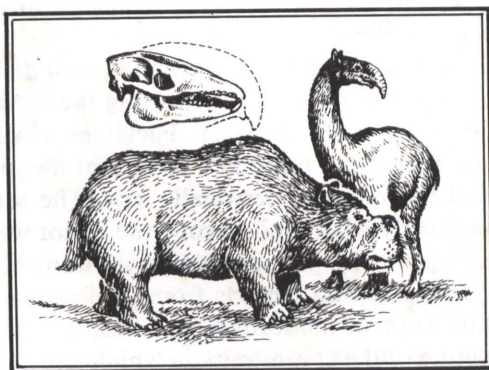
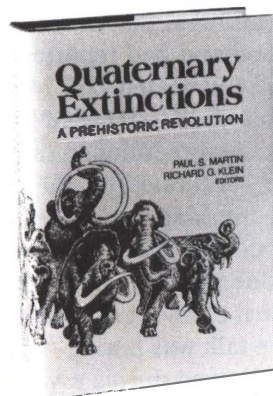
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